# VITALISM:

## ITS HISTORY AND VALIDITY

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*By*

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# PREFACE

The first requirement in a book about vitalism is to consider the meaning or meanings of this term.

A typical lay definition is that of the *Concise Oxford Dictionary*: "The doctrine that life originates in a vital principle distinct from chemical and other physical forces." But such definitions are too brief and vague to be satisfactory, Science only knows "life" in the concrete, in living things. The definition quoted suggests biogenesis, the fact that living things are produced only by already existing organisms. But this is inadequate, for every historical theory of vitalism implies not only origin but growth and metabolism through some distinctive agency which might be termed "vital principle."

Nor can precise definition be found in the allusions scattered thinly among the volumes of the latest (1930) edition of the *Encyclopaedia Britannica*, which has no article on Vitalism; reference must be made under *Biology*, *Chemistry*, *Holism*, *Mechanism* and *Medicine, History of.* According to these, vitalism is opposed to mechanism, was founded by Stahl, has died owing to death-blows, has a rather ghostly survival as a current trend in medical theory, is active in "emergent" and "substantial" forms, and presents pitfalls which can be avoided by holists; though Professor Wolf states that a "merely mechanical explanation" has been found inadequate to account for the facts of life (art.—*Mechanism*).

If vitalism be interpreted to mean the teaching of Stahl, it may indeed be considered dead or at least moribund. But the ideas of modern biologists such as Professor Driesch are not dead, and he uses the term "vitalism" in a wide sense to include not only his own limited, entelechial, theory but also all the various doctrines which, from the time of Aristotle, have described living things as actuated by some power or principle additional to those of mechanics and chemistry (*cf.* Part III(*a*) below).

This is the definition adopted in this book. As the last chapter shows, it covers at least three divisions of vitalism,

autonomy in living things generally, consciousness or something of that sort in animals, and independence of mind in Man Plants are fundamentally involved, particularly in biogenesis and resistance to entropy-increase But life is more actively and variously developed in animals, so the literature of the subject refers largely to zoology, including natural history and embryology Psychology and kindred studies come into this history because mechanism, if adequate, must account for all biological phenomena and not some only I assume, however, that vitalistic theories can be held concurrently with belief in the uniformity of chemical and physical "laws" as taught by modern science, though some of the older theories were not so held

Similarly, "mechanism" or "materialism" is used in a wide sense for the opposite theory There is much truth in Woodger's statement that vitalism and mechanism are the only two types of theoretical biology yet devised (*Biological Principles*, p. 441)

I use the first person singular when mentioning beliefs of my own which may not be accepted by all biologists, and "we" in reference to ideas which are held by at least the majority of educated people

Finally, I would express my thanks for suggestions for condensing and strengthening the original thesis to Professors E. W. MacBride, F R S , and E J Salisbury, F.R S.; for their courteous assistance in obtaining books, often ancient and valuable, to the Librarians and staffs of the University of London Library, the Science Library, the Linnean Society, the British Museum (Natural History), Raffles College, Singapore, and the Malacca Library; my acknowledgments to Messrs. H. K Lewis & Co. for supplying books overseas, and my special thanks to the Publication Fund of the University of London for a generous grant towards the cost of publication without which this book could hardly have appeared, despite curtailment in matters of detail which do not affect the original arrangement or argument.

L. Richmond Wheeler.

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# PART I

## EARLY PHASES OF VITALISTIC THOUGHT: WITH COMPARISONS, CONTRASTS AND SUMMARY

# I(*a*).—ANCIENT

"The analysis of the Aristotelian theory of life must therefore be one of the corner-stones of any historical work on biology."
H. DRIESCH (*History and Theory of Vitalism*, p. 11).

## (i) *Aristotle* (*Greek 386-322* B.C.) *: form, soul, entelechy.*

OF Aristotle as the founder of logic and psychology, as one of the greatest thinkers in the history of the world, even of Aristotle as a very great biologist, this is not the place to write. An excellent summary of his general work as a naturalist is given by Singer in *Greek Biology and Greek Medicine*, while Osborn discusses his position in the history of evolution in *From the Greeks to Darwin.* Here only a brief summary of the reasons for considering him as a vitalist can be given, with some critical survey of his vitalistic conceptions, which will be compared later with those of other thinkers (III(*d*) 2, v; *cf* II(*c*), i, for his errors concerning abiogenesis).

But it is important to remember the background of his vitalism—a supremely great power of original thought, an encyclopaedic erudition which has seldom been equalled, never surpassed, and the biological observations of one who, as Dr. Singer states, "is now universally admitted to have been one of the very greatest investigators of living nature" (*Greek Biology*, p. 21); while Darwin wrote in 1882, near the close of his life: "I had not the most remote notion what a wonderful man he was. Linnaeus and Cuvier have been my two gods, though in very different ways, but they were mere schoolboys to old Aristotle" (*op. cit*, p. 78). The combination of philosopher and outdoor naturalist is not common and should always command respect, especially when, as so pre-eminently in the case of Aristotle, it is combined with an unusually wide range of general knowledge and exceptional power of original thought.

Aristotle, Dr. Singer considers, "is in the fullest sense a vitalist" (*op. cit*, p. 27). This view is based largely upon Aristotle's idea of "soul" or ψυχή and the peculiar teleology

connected with this fundamental part of Aristotle's thought concerning living things.

The soul or "form" (*De Anima*, II, 1, 4), according to Aristotle, is an essential principle in the organism, which causes and controls its growth as a whole, and also the development, as means for definite ends, of its parts, such as the various kinds of teeth and the human hand (Singer, pp. 27-9; *De part. animalium*, i, I, 641). There is a close similarity between this conception and the entelechy of Driesch (*cf.* III(*a*), ii, below). The soul "is neither independent of, nor . . . identical with, the body." Their relationship "was determined by the existence of an *Entelechy*, an 'indwelling purposiveness'", the nature of which may be illustrated by a quotation from the *De Anima*:

"They are right who hold the soul as not independent of the body and yet as not in itself anything of the nature of the body . . . It therefore resides in body, and moreover, a particular soul to a particular body. . . For the Entelechy of each thing comes naturally to be developed in the potentiality [or matter—L.R.W] of each thing, and it is manifest that soul is a certain Entelechy and notional form of that which has the capacity to be endowed with soul" This, Singer concludes, "is the basic thought in Aristotle's biological work" . . . as "in much biological work at the present day" (E B., 3, 609; *De Anima*, II, 2, 14-15; Hicks translates entelechy by "actuality").

Aristotle distinguishes three main types of soul, corresponding to the broad divisions observable in nature between plants, animals and Man. The *vegetative* soul or "faculty of soul" is required for nutrition and reproduction, and is therefore found in plants and other living things; the *sensitive* soul or faculty is found in all animals, even the lowliest ones which appear to possess tactile sensation only, and, of course, in Man too; Man alone possesses the third, the *rational*, soul as well as the other two (Singer, pp. 40-41; *cf. De Anima*, II, 3, and Hicks, Introduction; and *De gen.*, II, 736b). A. R Wallace came to a similar though quite independent conclusion in the nineteenth century (*Darwinism*, Ch. XV; *cf.* II(*d*), ii, below).

But Singer considers that, though at first Aristotle separated Man sharply from lower animals, he having a rational soul and they not, as his knowledge increased he seems to have regarded this distinction as relative rather than absolute, or, rather, that animals possessed some degree of reason, in addition to the sensitive or animal soul possessed by them and by Man (E B , 3, 609) This accords with the views of some modern zoologists (*cf* III(*b*), 3, v, and III(*d*), 1, below, *re* Durken, E. S Russell, etc ). Actually, in the *De Anima* (Part II) Aristotle expounds several functions of "soul" above the first two, which are typical of plants and of all animals respectively. And this accords with the various modern arguments for autonomy in animals, culminating in independent mind in Man, summarised in III(*d*), 2, iii, and iv, numbers 6, 7, 10 and 11 Animals possess very varying powers of sensation and autonomy, but have more than plants and less than Man

Aristotle realised, moreover, that the different kinds of living things "can be arranged in a series in which the gradations are easy . . 'little by little from things lifeless to animal life' ", with many intermediate forms (*Greek Biology*, pp. 29-30; *De partibus*, IV, 5, 681a, 12; *Historia an* , VIII, 1, 588b). But this apparent modernity is qualified by his uncritical acceptance of abiogenesis (*cf* II(*c*), 1), and the four "elements" and their properties.

A peculiar quality of Aristotle's thought on this subject, due to the impossibility at his period of observing microscopic objects such as spermatozoa and mammalian ova, was his idea that the female contributed matter to the embryo while the male contributed nothing material, only the principle, soul, power (*De gen* , I, 19, and II, 1), or—to use a typically Aristotelian concept—form. Thus, he found nothing difficult in the cases in which the male does not appear to contribute actual semen or in actual examples of parthenogenesis. The semen, on Aristotle's hypothesis, being material, is actually of no importance; all that matters is the non-material form or principle contributed by the male. The inheritance of characters from the female as well as from the male he explained by considering that the embryo was produced from the catamenia, which

do not appear during gestation, they being "a secretion as the semen is" (*De generatione anim*, I, 19, 727a and 20). He was also, like other naturalists till the time of Camerarius, ignorant of the sexual processes in plants (*e g*, *De gen.*, I, 23).

The advance of knowledge concerning reproduction, sexual and asexual, which followed the invention and improvement of microscopes, has, of course, relegated this portion of Aristotle's philosophical biology to the lumber-room of abandoned, fanciful ideas But a passing word of tribute must be paid to his brilliant first-hand observations on such aspects of embryology as were perceivable in his day The *De generatione* was for some two thousand years the finest achievement in animal embryology, and many of the observations recorded therein, which were disbelieved by later biologists, have more recently been confirmed as strikingly accurate records of normal and abnormal phenomena in reproduction (*Greek Biology*, pp 35-8; Singer, *History of Biology*, p 34)

Aristotle's weakness as a physiologist has been laid bare by Singer. "He formally rejected the older views of Diogenes of Apollonia, of Alcmacon . and of the Hippocratic writings, that placed the seat of sensation in the brain" He thought that the heart had three chambers only, that it took in air direct from the lungs, was the central organ of the body; and so on (*Greek Biology*, pp 52-3) But he rejected the brain as the seat of sensation, as Dr MacBride says, for the sound practical reason that operations on the brain do not hurt. No man can be a master of all details of all branches of knowledge—not even an Aristotle But, as both a great philosopher and a "supreme naturalist by his natural endowments," Aristotle was bound to see that the whole is always greater than the part, nature greater than an organism, an organism greater than an organ or tissue; so he stands out in history as, among other things, the first great vitalist (*cf. op. cit.*, pp. 53-4).

Osborn notes "A marked chasm between his theistic, or dualistic, teaching and the sceptical . . . agnostic, and, to a certain extent, monistic teaching of Epicurus", who revived the outlook of Empedocles and Democritus. If Aristotle had

only accepted the survival of the fittest doctrine of Empedocles, "he would have been the literal prophet of Darwinism," Osborn considers, adding, however, that Aristotle's rejection of the survival hypothesis was a sound induction "from his own limited knowledge of Nature" (*From the Greeks to Darwin*, pp. 43-59, 64-8) But Singer, who is not a Darwinian as Osborn was, thinks that Aristotle never gained a clear idea of organic evolution, though he "was moving in that direction" (E B., 3, 610d)

It is interesting to read what Driesch himself says of Aristotle in his *History and Theory of Vitalism.* In his opinion the Stagirite is "the first exponent of a scientific 'vitalism'"; he is also "a typical representative of antiquity and the Middle Ages" and "a typical precursor of all vitalistic theories until the most recent times"; for the phenomena of co-ordinated animal movements and of formation from the germ have always been the starting-point of Vitalism (*op. cit*, pp. 11-12).

Aristotle's theory of development is epigenetic; heart, liver, and so on, come gradually into existence in the embryo, *e g.* the chick, which Aristotle studied carefully. Something potential arises because of something else which already exists. But, though the potentiality may be compared to the statue which is potential in a block of marble, the existent or "entelechy" to which its production is due is not the senseless material marble block, but the thought or concept of the statue in the mind of the sculptor. It is this thought or idea which answers to the "form" or entelechy of Aristotle (*op. cit.*, pp 13-14). Each part of a growing embryo arises *by* something actually existent (analogous to a Platonic idea) out of something potentially existent, like the possible statue in the marble block (*De gen. an*, II, i, 41[1], and *op. cit.*, pp. 15-16); or, parts arise out of the matter supplied by the mother because of the "soul" or "form" supplied by the male.

And the seed transmits to the matter of the embryo the soul or form by which it is itself animated (*op. cit.*, p. 17). Thus the "soul" (ψυχή) is the elementary principle (ἀρχή) of all things living, or, "the first actuality [as

[1] A reference I cannot trace in Platt's translation (Oxford, 1912)—L.R.W.

Driesch translates πρώτη ἐντελέχεια] of a natural body, having in it the capacity of life, and of one possessed of organs" (*op. cit*, p. 18)

The entelechy of Aristotle is then a power or directive "soul" within the organism, an "innate and inherent factor", as Singer says (E B, 15, 199b); adding that whatever theistic or "mystical" ideas may be found in his various works, the motive force of the organism is represented as something in the organism itself.

Dr. E. S. Russell, however, with whose ideas I am usually in close agreement, considers that Aristotle's view "was not vitalistic in the modern sense; it did not imply a dualism of matter and entelechy" because his "soul" expressed the activity of the organism as a whole (*Advancement of Science*, 1934, p. 84). But I think that Driesch and Singer are correct in claiming Aristotle as a vitalist; Russell does not seem to allow for the "form" which holds so large a place in the teaching of the Stagirite Still, from another point of view, Russell is yet another of the moderns who find in Aristotle a "first-rate naturalist and observer" (*op. cit*, p. 87), and, rightly or wrongly, a "father" of their own biological views.

Driesch agrees with Singer that Aristotle's theory of life is "pure vitalism"; and he emphasises that this theory arose "from an entirely impartial contemplation of life's phenomena, and not as the result of a struggle against other doctrines", which were held by Greeks before and after Aristotle. Indeed, he finds that Aristotle did not attach sufficient importance to the concept of necessity as enunciated by Democritus. And by his conception of "entelechy" he "created the link between idea and reality which is lacking in Plato" (*op. cit.*, pp. 19-21).

Finally, we are reminded of the tremendous influence which Aristotle exerted in biological as in other matters, despite an attempt in the thirteenth century, foiled by Albertus Magnus and Aquinas, to overthrow it, down to the eighteenth and even "for many" to the middle of the nineteenth century. Since then his reputation as a field naturalist and embryologist has been enhanced (Singer, *loc. cit.*); and Driesch claims that Aristotle's "assertions",

too, have been justified by recent research. This claim will be considered in due course (*cf.* III(*d*), 2, v).

(ii) *Decline of Greek biology.*

With the death about 287 B.C. of Theophrastus, the botanist who succeeded Aristotle as head of the Lyceum, "biological science substantially disappears from the Greek world" (*Greek Biology*, p. 61). Osborn, too, remarks upon the rapid decline in Greek productiveness, leading to its extinction, after its highest level was reached in Aristotle (*op. cit.*, p. 57). In fact, for the student of vitalism there is little worthy of note in the history of human thought after the death of Aristotle in 322 B C. until we come to the man whose writings influenced general beliefs and medical practice and theory in Islam and Christendom till well into the seventeenth century A D. This man was Claudius Galen of Pergamon.

Still, the Alexandrian School, which arose soon after the death of Aristotle, produced advances in medical knowledge, and one of its greatest teachers deserves notice before Galen is considered.

Erasistratus of Chios, *c.* 300 B C., unlike Aristotle and Galen, "professed himself a rationalist." Yet he comes into the line of vitalistic biologists In contrast to Aristotle's internal entelechy he invokes "Nature" as an external directive power to explain the working of the body; but this conception, though vague, implied more than blind mechanistic forces. He revived the "pneumatism" which some earlier thinkers had put forward, and which became distinctive of Galen's physiology. But for Erasistratus there were only two spirits or *pneuma-s* in the body. Air breathed through the lungs reaches the heart and combines with the blood to form "vital spirits", which go to the body through the arteries. That portion which reaches the brain forms "animal spirits", which fill the cerebral ventricles, passing to the body along the nerves. The veins, he considered, carried blood only, not blood and the "natural spirits" which Galen thought were formed in the liver. Like Galen, he attributed the movements and higher functions of the body to "animal spirits", conceived of as a vital fluid of a gaseous rather than

a liquid kind. The shortening of muscles he believed due to distension by this fluid (*cf* Singer, E.B, 15, 199).

(iii) *Galen (Greek*· A D *131-201); animal and other spirits; vital fluid*

Galen "was the greatest biologist of the late Greek period", "a naturalist of great ability and industry, and knew well the value of the experimental way" (Singer, *Greek Biology*, pp. 65-6) His total output of writings was enormous and, unlike many Greek medical and biological works, his have been preserved Also many later books which appeared under other names were little more than translations or compilations from Galen's vast and numerous treatises

His primary interest was medical; but, though he examined a human skeleton at Alexandria, "his physiology and antomy had to be derived mainly from animal sources" He also studied the writings of the various medical schools which developed after the foundation of the great University of Alexandria in 300 B C From these and from his observations and dissections of animals Galen built up that physiological teaching which was to dominate the medical, scientific and popular ideas of the working of the human and animal body till well into the seventeenth century (*Greek Biology*, pp 65-6)

This system is well summarised by Singer (*op cit.*, pp. 66-70) According to Galen, life depends primarily on breathing, though, of course, he has no conception of the existence or function of oxygen; for him respiration meant a taking in of spirit (Latin *anima*, Greek *pneuma*) from the general world-soul. This primitive spirit passes to the lungs through the trachea and thence to the left venticle through what is now termed the pulmonary vein.

Digested food, absorbed as chyle from the intestine, is carried by the portal vein to the liver, which Galen, in opposition to Aristotle, who looked on the heart as the chief organ, thought provided the "innate" heat of the body. In the liver the chyle was elaborated into venous blood and endowed with the lowest or simplest of the three kinds of spirits produced in a living animal body and essential for its health. The "pneuma" originating in the liver is the *natural*

*spirits*. This, with the food material from the intestine, is carried to the body by the first of the three distributing systems, the veins In these, as in the arteries and the supposedly hollow nerves, the fluid ebbed and flowed continuously throughout life The liver was the centre for the veins, the heart for the arteries, and the brain for the nerves, from each the appropriate "spirits" were sent to the body, not by a circulation but by an ebb and flow up and down a single series of channels

But some of the venous blood from the hepatic vein of the liver went not to the body at large but to the heart, entering the right side of the organ Here it parted with its impurities, the "exhalations" of the organs brought back by the ebb in the veins, and these were carried off by what we call the pulmonary artery to the lung and thence expired into the air. Most of the purified venous blood returned to the venous or liver-vessel system

Now came another characteristically erroneous and correspondingly important part of the Galenic system. Some of the venous blood in the right ventricle was believed to pass through the septum by invisible (actually non-existent) pores into the left ventricle Here it mingled with the *pneuma* from the lungs and its natural spirits were thereby elaborated into a higher form—the *vital spirits*: these were distributed by the ebb and flow in the arteries

But the arterially conveyed vital spirits that reached the brain suffered a further elaboration in that organ, resulting in the formation of the *animal spirits* This supposedly ethereal substance travelled to all parts of the body along the supposedly hollow nerves Galen's minor errors in thinking that the arterial *rete mirabile* of the calf and the hepatic vein of the dog also occurred in man are of little importance here.

The points of interest to students of vitalism are three:

(*a*) The vitalistic conception of the three "spirits";
(*b*) Galen's teleology;
(*c*) The profound influence he exercised for so long and the harm done to anti-mechanical views by the collapse of his ill-founded ideas.

(*a*) The vitalistic nature of his teaching is as obvious as its entire lack of sound foundation.

Breathing is essential to all living creatures except anaerobic micro-organisms, but not in Galen's way. A world soul which is appropriated through gills or nostrils is unknown to science or to modern philosophy The gross errors of the three sets of vessels, each with its ebb and flow, have been exposed as completely as the subsidiary mistakes of regarding nerves as hollow tubes and the ventricular septum as being perforated But as long as the body was believed to build up the three increasingly non-material spirits—natural, vital and animal—from blood, digested food and inspired world soul, it was obvious that man and animals at least were beings utterly outside the reign of whatever laws controlled phenomena in the non-living portions of nature. Quite apart from the influence of Aristotelian ideas, all medical men and biologists from A.D. 200 down to A.D. 1628 or longer were vitalists because they were Galenists.

(*b*) The classical examples of Descartes and Newton—to name no others—show that able men can develop mechanical ideas about at least a part of natural phenomena and yet also hold firm beliefs in the activity and beneficence of a Supreme Being But Galen not only saw the action of an omniscient and omnipotent Creator in all things, like Paley and A R. Wallace in his later years, but taught this as part of his science. This commended his teaching to the followers of the three great monotheistic religions—who alone matter seriously in the history of science, whatever their periodic shortcomings—and so secured its survival (*op. cit*, p. 70). And it reinforced powerfully the vitalistic trend of his teaching; for vitalism has usually been connected with theism just as its opposite is the only theory that can accord with atheism (*cf.* II(*b*), iv, and III(*d*) 2, below).

(*c*) Enough has been said of Galen's enormous influence down to the seventeenth century. Even Vesalius, acclaimed by Foster, Locy and others, as the destroyer of authority and the founder of new methods in physiology and biology generally, did not do more than hint that Galen's ideas about spirits and their conveyance to the body were wrong; he was "content to teach" them. Sir M Foster's *Lectures on the History of Physiology* are illuminating; not only Vesalius

and Fabricius before Harvey but Descartes, van Helmont, Mayow and Willis, after his proof of the circulation of the blood, continued more or less the secular Galenic tradition (*op. cit*, pp 12-13, 38, 59, 134-9, 196, 253-6, 271-5). But as his doctrines became generally discredited so the ideas associated with them tended to suffer (*cf.* I(*b*), 2, i, and II(*b*), ii).

No recrudescence of Galenism in any shape or form can be claimed to-day, though Aristotle never stood higher whether as a biologist, by general consent, or as an exponent of vitalism according to Driesch and other vitalists. Galen's physiology was hopelessly and demonstrably wrong, so the basis for his theories has disappeared; the bases for the conceptions of Aristotle remain, apart from his ignorance about sexuality and biogenesis, so the conceptions are at least possible (*cf.* III(*d*), 2, v).

(iv) *William Harvey (English: 1578-1657); vital fluid hypothesis destroyed by his discovery of the circulation of the blood; vitalistic views on embryology; epigenesis.*

We have seen how Galen's teaching about the three kinds of spirits was accepted till well into the seventeenth century, with all its implications of special powers in the body, which created these spirits, and of the need of the body for their vivifying action to enable its natural functions to be carried on.

Accounts of Servetus, Caesalpino and others, who, in varying degrees, anticipated Harvey's great discovery, can be read in any text-book of the history of science, for instance, in Foster's *Lectures*, Chapter II We are not concerned with them here. They failed to convince their fellow doctors and tudents of physiology. Harvey succeeded because his work was based on experiment and observation welded together by clear, simple reasoning; yet even his book, *De Motu Cordis et Sanguinis*, was received at first with considerable opposition, and for long after its publication in 1628 Descartes and other famous writers continued to follow part at least of Galen's doctrine.

What Harvey did was to show to any unprejudiced reader that, first, the systole or contraction of the heart forced blood

into the arteries; secondly, that all the contents of the right ventricle went to the lungs—none directly into the left ventricle—and then back through the left auricle to the left ventricle—the pulmonary circulation; thirdly, that all the blood was driven from the left ventricle along the ordinary arteries to the body and thence returned through the veins to the right auricle to begin the double circulation again. All he disproved immediately was the supposed ebb and flow along two supposedly unconnected sets of vessels—the arteries and the veins, and the supposed function of the liver as the centre for the venous system.

As Foster well points out (*op. cit.*, p. 46), quoting one of Harvey's most famous sentences, Harvey simply disregarded the current notions about spirits, saying that the question as to whether the heart gave "heat, spirit, perfection" or anything else except motion to the blood must be postponed and "decided upon other grounds." "Yet his demonstration was the death-blow to the doctrine of the 'spirits.'" There was one blood supply to the body, not two: the basis for the separate supplies of "natural" and "vital" spirits was destroyed, and the belief in these two spirits could not exist without it. But, of course, Harvey's work did not directly affect the possible elaboration of some sort of animal spirits in the brain nor their possible passage along nerves, hollow or solid, to the body; so the belief in some sort of special nervous spirits or current lingered on, gradually merging into the modern belief in non-material impulses which pass along the nerves. Willis (1621-1675), for instance, who advanced knowledge of the brain, used very Galenic phraseology when describing the action of the nervous system (*op. cit.*, pp. 269-74), and "as late as 1796 competent anatomists were still identifying the fluid in the ventricles of the brain with Galen's 'animal spirits'" (Whetham, *Hist. of Sci.*, p. 274). On the other hand, the overthrow of the natural and vital spirits in the blood system could not but make men sceptical of the supposed formation of animal spirits in the old sense of the term. The term "vital spirits" might be used by Willis and his contemporaries, but the spirits had become "mere qualities of the blood, their names might be retained, but the virtue had gone out of the names" (Foster, p. 52).

The above summary gives the usual view of historians of biology, such as the late Sir Michael Foster, on whose *Lectures* it is largely based, upon the part played by Harvey in causing the abandonment of a crude vitalistic view of a definitely mechanical problem—the double circulation of the blood—by a rigorously mechanical treatment (*op. cit.*, p. 46). And with this usual view I agree—as far as it goes.

But as Driesch in his brief outline of the *History of Vitalism* ignores Galen it is interesting to examine his account of Harvey Since Galen's theory of "spirits" is not mentioned, there is, of course, no need to refer to its overthrow through Harvey's physiological discoveries. He is considered here not as the opponent of an erroneous form of vitalism, that of vital fluids, but as the exponent of the Aristotelian type of vitalism (*op. cit.*, pp 26-30), and the ground for this view is found in Harvey's less famous work, *De generatione animalium.* Its preparation, in which King Charles I took considerable interest, occupied his leisure between 1628 and 1651, when it was published by Harvey's friend Ent It is five times as long as "De Motu," and lacks the careful arrangement of that outstanding treatise. Here Harvey originated the view that all higher animals have a single type of *origin*—an egg—a view summarised, though not by Harvey himself, in the historic phrase, *omne vivum ex ovo* Driesch states that Harvey "was by no means opposed to the theory of spontaneous generation" (*op. cit*, p 20); Dr. Singer and his colleague state that "he declares against spontaneous generation" (E.B, 11, p 237d). T. H. Huxley shows that Driesch's view is the correct one, as Harvey meant by "egg" not what we mean but any small particle of organised or living matter (*Essays*, Everyman's Library, p 200). Wolf agrees with Huxley (*Hist. of Sci*, I, p. 420), and the point is settled by reference to Harvey's own Latin and the translation by Willis.[1] Spontaneous generation was an almost universal belief from the time of Aristotle down to Harvey and even later (*cf.* Part II(*c*) below). Much of his work was soon superseded by Malpighi's beautiful researches. But, *inter alia*, Harvey corrected Aristotle's error about form and

[1] G. Harveii Opera Omnia, MDCCLXVI, pub Coll of Physicians, p 182, Works of W Harvey, trans Willis, Sydenham Soc, Lond. MDCCCXLVII, p. 170.

matter, maintaining that both parents contributed to the fertilised egg. He made a brilliant prophecy of the later discovery by von Baer that Man and mammals develop from eggs like other vertebrates He disproved Aristotle's idea that the catamenia supplied the "matter" which the female contributed to the embryo; but he agreed with the Greek biologist in ascribing to the animal *ovum* a vegetative and a sensitive soul. And he stood for epigenesis, the gradual appearance of new organs in succession, as against preformation, the idea that all organs are present in miniature in the ovum, though this conception was advocated by many, especially in Germany, in the following century.

Of more importance for our present purpose is that he regarded epigenetic development as due, not to mechanical laws but to a definite life power, an "opifex" or master builder, a "principium," an internal principle or force. Where Harvey tries to go further than Aristotle in explaining the action of this principle and of a higher "soul" he is not easy to follow. But he tries not only to describe the processes of animal development, notably in the chick, but to explain the reason for this amazing phenomenon, so utterly unlike anything witnessed in the inorganic world. And so Driesch is correct in claiming that Harvey's explanation is a "peculiar vital autonomy"—and that Harvey is a true link in the chain of vitalistic thinkers which begins with Aristotle, if not indeed still earlier with the authors of the nature psalms. Singer considers his treatise the greatest work on embryology between those of Aristotle and von Baer (1796-1876) (*Greek Biology*, p. 77), though this seems hardly just to C Fr. Wolff.

Harvey indeed is as significant in the history of biological theory as in that of biological discovery When dealing with particular phenomena—the working of the heart and the movement of the blood—he is the typical man of science in the strict sense of the word. He sticks to observation and experiment and ignores theories about "spirits" and semi-metaphysical questions. This is the Harvey rightly celebrated in medicine and biology and dear to all those who like to see ill-founded ideas swept away by logical methods; from a narrow point of view he may even be made to appear

as one of the many who are described as dealing "death-blows" to vitalism at various times and in various ways. But the appearance and development of a new organism in an indifferent physical world is quite a different thing from the isolated study of the action of one of its organs. So in the *De generatione* we see another aspect of the same great man, still scientific but here groping for adequate explanation not of a mechanical problem but of an intricate system of inter-related phenomena. Few great biologists can be scientific only. They are drawn into philosophising, whether they wish it or not. Such was Harvey when he studied the evolution of an organism as a whole; and the philosophy he combined with his science was that of one who finds that mechanism is not enough, however difficult may be the formulation of an adequate hypothesis.

# I(b).—THE SEVENTEENTH AND EIGHTEENTH CENTURIES (AFTER HARVEY)

"Naturam expellas furca, tamen usque recurret."
HORACE (Epistles, I, 10).

CHAPTER I

## SPECIFIC VITALISTIC THEORIES UP TO STAHL

### (1) *R. Descartes (French: 1596-1650); the body as a machine; and the seat of the soul*

ONCE again we come to a man of outstanding importance in the history of science, "a great mathematician . . . an accomplished physicist . . . above all . . . a philosopher." But, unlike most of those who play an important part in this history, he was not a biologist (*cf* Foster, *Lectures on Physiology*, p 56)

Nevertheless, *L'Homme* (pub 1662) was "the first text-book on physiology" (Foster, p 57; Singer, *History of Biology*, p. 354); it was written because "it was part of his philosophy to show that man consisted of an earthly machine inhabited and governed by a rational soul." Much of its teaching was retrograde, yet its general ideas influenced physiological thinkers long after Descartes had passed away and physiology had made much progress in matters of detail (Foster, p. 56). Descartes did not publish it while alive for fear of conflict with the Church, which had lately condemned Galileo's teaching. And the "machine" which he discussed was not the human body itself but one exactly like it! (*Tractatus de Homine, Pars Prima, I and II.*)

He gave a "refutation" of Harvey's *De Motu Cordis* in his *De Formatione Foetus, Secunda Pars*. He admitted Harvey's discovery of the two circulations but not the "keystone" of Harvey's argument—the propulsion of the blood by the systole of the heart; and he maintained the ancient idea of

the innate heat of the heart as causing the expansion of the blood that enters it But his physiology was concerned mainly with the working of the brain and nervous system

"The most agitated and vivified parts of the blood, being carried to the brain by the arteries constitute . . a very subtle air or wind, called the animal spirits, which dilating the brain fit it to receive the impressions of external objects and also those of the soul, that is to say, fit it to be the seat of common sensation, of imagination, and of memory. This air or these spirits then flow from the brain along the nerves into all the muscles, whereby they dispose the nerves to serve as the organs of the external senses, and finally distending the muscles give movement to all the limbs" (quoted by Foster, *op cit*, p 59)

So Descartes uses Harvey's teaching about blood circulation and Aristotle's ideas of innate heat in the heart and animal spirits, together with purely imaginary details of his own, for his own purpose, which is to show that the human body works as a machine to which a rational soul, comparable to that of Aristotle, is added. The relevant details of his exposition are given fully by Foster (*op cit*, pp. 258-66) and need not be repeated here. He conceives of the nerves as hollow tubes along which the fluid, gascous, animal spirits pass to distend muscles and so make the limbs move But, also, these supposed tubes have a fibrous marrow (corresponding to the modern idea of a nerve); sensation is conveyed along these fibres centripetally to the brain. In response to these sensations the animal spirits in the brain are stirred to action and flow down the nerves to produce reflex action.

This is mechanistic—put a penny in the slot and a packet appears at the opening Yet, in his way, Descartes was a vitalist; he was a vitalist as regards Man but a mechanist regarding animals and, of course, plants As Singer well points out (E.B., 7, 249), there is no reason to doubt his sincerity not merely as a Christian believer but as an orthodox adherent of the Roman Catholic Church during one of its most dogmatic periods. Not merely the existence but the essentiality of GOD for the creation and maintenance of nature and of the spirits of men was the cardinal point in his philosophy (*Discourse of Method III and V, Principles of*

*Philosophy—Appendix: Propns I-III*) So, though animals might be explicable on mechanistic grounds, for Descartes Man is entirely different He possesses a soul, which is rational as well as immortal, and is wholly inexplicable by mechanistic arguments.

Descartes thinks out for himself a connection between soul and body. The seat of the soul is the pineal gland. The sensations received along the nerves by the animal spirits in the cavities of the brain not only affect the rest of the brain to effect reflex actions but also impinge upon the pineal gland and so put the soul in contact with its material surroundings and give rise to true "ideas" Similarly the soul's ideas can be translated into bodily action So the soul can act on its material surroundings through its bodily machine; and the actions of this machine are due sometimes to impulses caused by the thoughts or wishes of the soul, located in the pineal gland, sometimes to its own automatic reactions to external stimuli (Foster, pp 264-5)

But, as physiology progressed, the pineal gland was seen to be no more suitable for the location of the soul than the duodenum (*cf.* Van Helmont below); and though Descartes' general exposition of the functioning of the nervous system does not require very drastic alteration to adapt it to modern views (Foster, p 266) the alteration would abolish the animal spirits in the brain cavities which formed a link between the pineal soul and the body In an opposite direction, few physiologists have appreciated the philosophical reasons which led Descartes to believe in the rational soul itself as an entity of indubitable authority. And his dualism of body and mind (or soul) ultimately depended upon the constant intervention of GOD to harmonise the functioning of two radically different things. Consequently, it became only too easy to omit the spiritual side of his dualism altogether and turn his system into a monism of a strictly mechanistic type (*cf.* E B. 7, 249c, 251d); hence his powerful influence directly encouraged an anti-vitalistic outlook in biology. Driesch in his *History of Vitalism* mentions Descartes as, with Leibniz, adopting a mechanistic view of life, except, however, for the soul of man.

It is beyond our present purpose to do more than briefly

observe that, by an opposite process, the exaltation of his doctrine of the supreme importance and independence of the human rational soul tends to lead to idealism in philosophy, even possibly to solipsism; though Descartes' religion and science saved him from these (E B 7, p. 248; *cf.* III(*b*), 1, below).

In his address to the British Association, Zoology Section, 1934, Dr. E. S Russell ably outlined and criticised the baneful effect which Descartes' well-intentioned dualism has had upon biology long after other sciences have adopted a more synthetic attitude Thus in 1887 T H Huxley put forward the "postulates" and "laws of Nature" of nineteenth-century materialism as the basis of physical science (*Methods and Results*, 1893, pp, 60-61), and placed the consciousness of "brutes" at least as an epi-phenomenon or "collateral product" of the working of "the mechanism of their body" (*ibid*, p. 240). This idea of the animal as "a physiological automaton" was "explicitly stated by Descartes in his *Discourse on Method* Pavlov stated, "Our starting-point has been Descartes' idea of the nervous reflex" (*Conditioned Reflexes*, 1927, p 7). Thus, as Russell says, "this abstract dualism has saddled us with the theory that the organism is a machine . . . the scientific study of behaviour thus becomes divorced from natural history and ceases to take its rightful place as an integral part of zoology" (*Advancement of Science*, 1934, p. 87).

Many forms of vitalism rose and fell after Descartes; but the mechanistic theory of life has continued to draw inspiration from his teaching, as Henderson states (*Fitness of the Environment*, p. 284). In Part III(*d*) I shall endeavour to show that the ideas of Descartes about biology are about to follow those of Aristotle concerning physics into the historical niches suitable for theories which have retarded progress in particular sciences for hundreds of years.

### (ii) *Van Helmont (Dutch: 1572-1644) ; blas, etc.*

Van Helmont's mental ancestry goes back at least two generations, as is well shown by Foster, whose *Lectures* (V) supply the main background for this section. Valentine was a Benedictine monk who taught many things about alchemy

and physiology in the latter half of the fifteenth century. He "apparently" founded the idea of the three "elements'—salt, sulphur and mercury—which dominated chemical thought for a century (*cf* R Boyle's *Sceptical Chymist*, which criticised Valentine's views) More important for the history of vitalism was his notion of certain "embodiments of energy" or psychic forces through which the Supreme Being governed chemical changes and other natural events; these he termed *archaei*. The famous Paracelsus (Swiss, *c*. 1490-1541) also taught many things, some utterly false or patently absurd, like the transmutation of base metals into gold (*Cambridge Readings in the Literature of Science*, pp. 75-83), others, like the use of laudanum, useful and true. But his cardinal ideas were those of Valentine—the three elements and the *archaei* There is a chief archaeus—"'that exalted invisible spirit, that occult virtue which is the artificer of nature in everyone'", there are minor *archaei* too In health the chemical processes of the body "are rightly governed by the *archaeus*"; during disease it fails; at death it is lost.

Here is a theory supposedly independent of the two great traditional systems of Aristotle and Galen as of the then modern advances of Vesalius and Harvey It was revived and developed by Van Helmont, who studied widely in his youth, concentrating finally on medicine and chemistry. Like Descartes, he was a devout Roman Catholic, and his chief physiological work only appeared after he was dead. He was influenced by Vesalius, not by Harvey or Francis Bacon; but much by Paracelsus He made interesting experiments in chemistry and botany, especially concerning "gas" and water, which he substituted for the three elements of Valentine as the constituents of all things; he made fermentation the basis of physiology, and had many sound ideas on nutrition. Despite Harvey's teaching, he believed in the perforated cardiac septum, which was strangely retrograde; but he was the first to deny the existence of "animal spirits"—a great advance in thought. Muscles, for him, were nourished by "vitalised blood" not by cerebral vapours.

To all this rational science, imperfect or not, he united

one of the many fantasies which have tended to discredit vitalistic ideas generally.

In addition to a sensitive soul, properly speaking found in man only, like Descartes' rational soul, he conceives of subordinate *archaei* or vitalistic forces, which he terms *Blas*. Various blas or archaei function during the various stages of digestion and assimilation. A *spiritus vitalis* in the left ventricle imparts vital spirits to the blood which flows through the arteries and also passes through the septum into the right ventricle to begin the vivifying process there! The result is described as changing the blood "into the vital spirit of the archaeus" (Foster, pp. 137-41) Ferments directly cause bodily changes, they are controlled by minor blas or archaei, these by a chief archaeus, that in its turn being subordinate to the sensitive soul, "the prime agent of all the acts of the body."

This soul controls movements and sensations by means of the brain and nerves but is actually seated in the pylorus, near the heart and stomach Yet it is "not there in a local manner" but like light in a burning candle. There is, further, an immortal mind (the "soul" of religious phraseology) to which the mortal sensitive and motive soul is as it were a husk or envelope, and this *mens immortalis* controls the sensitive soul as the latter does the various blas (*op. cit.*, p. 142).

The difference between the theories of Descartes and van Helmont is that, while both postulated a soul or mind as supreme in man, in Descartes it was so distinct from the body that later thinkers could discard the soul and retain Descartes' very complete picture of the body as a self-acting machine. But such a separation was impossible for van Helmont's ideas; take away all the blas connected by grades with the sensitive soul and its indwelling immortal mind and no causes for the simplest bodily functions remained. Hence, bizarre and fantastic as van Helmont's views are in themselves, they are in the line which led to the later, more refined, ideas of a vital principle or force as a form of energy similar to chemical and electrical energy and the like (*cf.* III(*b*), 3, iv).

But both these teachers confined their vitalism or animism to man; plants and animals for both were mechanisms, except that van Helmont allowed that they possessed a

certain vital power which might perhaps be a "forerunner of the soul" (*op cit*, p. 141)

Driesch considers that, though van Helmont condemned Aristotle as "ridiculous and ignorant of nature," his idea of the archaeus controlling the course of development is "really and unmistakably the Aristotelian teaching—only less profound" (*History of Vitalism*, p. 25) His quotations from van Helmont appear to support this view to some extent; but, broadly speaking, it is difficult to see any close similarity between their views; and the details are utterly different. Van Helmont rushes in where Aristotle does not tread.

(iii) *G. E Stahl (German : 1660-1734); "vitalism"*

Stahl is best known to scientists as the originator of the phlogiston theory, which dominated chemistry till its overthrow by Lavoisier a hundred years later But though "an accomplished chemist" (Foster, p 166), he was also a Court physician and a professor of medicine

Sylvius had opposed van Helmont's physiology of blas and archaei "and maintained that the events of the living body were ordinary chemical events", just as Borelli had shown that many of them, at least, could be treated in a mathematical or physical way (*cf* Ch. 2, i, below)

But Stahl, though omitting all the minor vitalistic forces of van Helmont—the blas and archaei—revived the fundamental ideas of his sensitive soul and immortal mind in a simpler way which took account of the slowly increasing chemical knowledge of the time Stahl taught that the sensitive soul was itself the immortal spiritual part of man, which also, however, "presided over" or directly controlled all chemical or physiological changes in the living body Hence, such changes were essentially different from any corresponding reactions which could be obtained by experiment in the laboratory (Foster, p. 167).

In addition to these two differences, primarily concerned with man—the agreeable absence of archaei and the single soul as opposed to the mortal soul centred in the pylorus with its distinct immortal kernel, Stahl's views differed from those of van Helmont in another important particular.

Van Helmont, rather like Descartes, drew his boundary principally between man on the one hand and all the rest of nature on the other Stahl's "fundamental position" should appeal to all biologists He held that the important division came between living things, simple or complex, and non-living things (Foster, p 167) Nevertheless, his arguments are concerned almost entirely with man, as is natural for a physician

He taught, very properly too, as far as it goes, that all living things, though capable of change, maintain their identity for a definite period, whereas non-living things that undergo change do so without any counteracting power of stability. He further insisted that the living body does not exist for itself but for the indwelling soul, which controls metabolism (though Stahl does not use this modern expression) for its own "uses and ends" Hence, "vital activities . are truly organic acts carried out in corporeal instruments by a superior acting cause" and "cannot . . . have any real likeness to such movements as, in the ordinary way, depend on the material condition of a body and take place without any direct use or end or aim" (Foster, pp. 168-9, from Stahl's *De mixti et vivi corporis vera diversitate*).

There remains a difficulty which is still unsolved. Either the human mind or spirit is an epi-phenomenon, a sort of by-product of bodily phenomena, as Huxley suggested, thought "a secretion of the brain as bile is of the liver" (K Vogt), or thought, mind, soul, are independent. If this is so, the correlation between mind or soul and body requires explanation or, at least, description. Their occurrences may go on independently, but simultaneously, without understandable connection—unless, as Descartes thought, God orders this psycho-physical parallelism. If not, there is a connection. Descartes tried to give a physical explanation, though he only centred the difficulty inside the pineal gland. Van Helmont interposed a hierarchy of archaei. Bergson and Eddington (*vide* III(*b*) 2, iii and iv) suggest that the connection occurs between spirit and the cerebral cortex. But still somehow, somewhere, it must occur. Stahl wisely discarded the earlier crude attempts of his own century to explain the bridging of this gap and proposed motion as the

solution "By motion indeed the soul carries out all its doings"; by motion the soul, which is diffused over the whole body, controls and modifies all the physical and chemical changes that admittedly occur in the body (Foster, pp 169, 171, 295).

It is usual to speak of Stahl as a vitalist, even as the founder of the vitalistic school in physiology (*cf.* Singer, E B. 15, p. 201d). Foster (p. 171) and Driesch (p 36) term him an "animist"; but Driesch admits that the distinction is not of great importance.

It is easy to dismiss Stahl's theory as mere imagination, or at least as wrong in some parts and obscure in others. The point is that for Stahl the body was exactly the reverse of a machine, and he used all his influence as a Court physician and a professor to uphold this view in his *Theoria medica vera* Driesch considers his "The first great scientific system of theoretical biology after Aristotle" It was, he says, free from mysticism (though Singer, *op cit*, takes the opposite view), and it was planned on a generous scale with "pre-vision of all logical consequences arising out of his views"; though he "neglects everything which is in any way inconvenient to his theories" (*History of Vitalism*, pp. 35-6). Driesch, however, sees no advance beyond Aristotle's views, rather, indeed, a retrogression Singer rightly views Stahl's ideas as, in the main, a return to those of Aristotle; the sensitive soul of Stahl is the *psyche* of the latter, though, as Driesch says, with the vegetative and sensitive parts left out and only the *anima rationalis* left (*ibid*, p 32).

Stahl's vitalism was largely based upon erroneous ideas about bio-chemical happenings, and so became liable to the destruction which overtook such views later (*vide* II(*b*), i); he made little use of embryology or morphogenesis or of the study of organisms in nature (*cf.* III(*d*), 1 and 2, iii), while some arguments now available for vitalism, such as entropy-resistance and biogenesis, were unknown in his day. His work is therefore very incomplete even in comparison with that of Aristotle (*cf.* III(*d*), 2, v). But, though his arguments for animistic vitalism are largely wrong in fact, Stahl showed an appreciation of the subtleties of metabolism which modern bio-chemists sometimes miss (*cf.* III(*b*), 3, iv); and he was a

noteworthy exponent in an age of growing mechanism of the great doctrine of the independence of the human mind which modern psychology has revived (*cf* III(*b*), 2, iii, and III(*d*), 2, iv) And his insistence on the identity amid change of living things and their distinctness from the inorganic are important vitalistic ideas which later mechanists failed to appreciate, and which redeem his vitalism from the charge of being purely "animistic" (*cf.* Rádl, *Geschichte der Biologischen Theorien*, i, 79 *seq.*).

CHAPTER 2

# OTHER INFLUENCES UPON VITALISTIC THEORY, FROM HARVEY TO A.D. 1800

## (i) *The development of mechanics and chemistry in the seventeenth century: Newton, Borelli, Sylvius.*

At the Renaissance Aristotle, like other Greek authors, became more widely known, and in biology his influence strengthened vitalistic ideas. But Harvey's studies in embryology were less known that his work on the heart and the circulation, and this had a mechanistic effect upon his contemporaries in the seventeenth century.

Meanwhile, other branches of science began to develop or come into being Despite great opposition, Galileo (Italian, 1564-1642) founded modern mechanics, and incidentally showed that in at least one field of thought Aristotle had been a very erroneous guide Though Copernicus had published his *De Revolutionibus* in 1543, the year of his death, its implications began to be generally understood only after the complementary discoveries in astronomy of Galileo, Tycho Brahe (1546-1601), and Kepler (1571-1630).

Long before the close of the seventeenth century the genius of Newton (English· 1642-1727), through independent discovery and the correlation of previous work, established the laws of mechanics and of astronomy which were unquestioned till the twentieth century, and, with them, a tremendous conception of uniformity in nature which became accepted by all educated people. The Supreme Being and Creator was acknowledged; but it was shown that—in inorganic nature at all events—He acted indirectly, so to speak, through sequences of antecedents and consequences which were, apparently, unchecked by any divine interference. These conclusions were inextricably connected with mathematics, of which all men recognised the univer-

sality, though then, as even now sometimes, they did not realise its abstract nature

Descartes' dualistic attempt to combine belief in the independent mind or soul of man with a rigidly mechanical view of physiology, especially of the brain, has been dealt with. Others adopted the more restricted method of Harvey, which was to deal with some definite part of the body and its functions and to show that these could be described successfully without recourse to supposed vital forces or spirits. The laws of mathematics and physics were first applied in this strictly scientific way, free from the false assumptions which Descartes made to complete his synthesis, by Sanctorius (1561-1636) and soon afterwards by Borelli (Italian 1608-1679) and his followers, who became known as the iatro-physical school.

Borelli was primarily a mathematician and a physicist, but he viewed physiology as a "part of physics" and dealt successfully with some of its problems and less successfully with others on purely mechanical lines His interesting friendship with Malpighi influenced him to consider biological matters His great book, *De Motu animalium*, was not published till after his death Foster gives an interesting account of him in his Lecture III

Incidentally it is worthy of remark that posthumous publication of the leading works of scientific writers was common at this period; others are. Descartes—*L'Homme*, van Helmont—*Ortus Medicinae*, Swammerdam—*Biblia Naturae*, Camerarius—*De sexu plantarum* The brilliant work of the Rev. S Hales (1677-1761) in animal and plant physiology deserves brief mention for its successful application of mechanics to numerous suitable problems

Long after mechanics and astronomy had reached substantially the form they maintained till the end of the nineteenth century progress in chemistry was slow and fitful (*cf.* 1, ii above).

But all the chemical science available in his day was applied by Sylvius (German: 1614-1672) to the study of physiology. He made no great discovery; but he was an able teacher, a follower of Harvey, and a learned man. As a chemist and physician he continued van Helmont's work

in physiology on the chemical side, with minor differences which do not concern us here (*cf* Foster, Lecture VI). But he broke away completely from the latter's views on archaei, sensitive soul, and so on Thereby he did good service to science; there is much in physiology, plant and animal, which can be studied by chemical methods, as the progress of modern bio-chemistry shows, and no vitalist can object to the application of chemical science as far as possible to the study of living organisms. The history of vitalistic thought abounds with illustrations of the need for constant correction by comparison with chemical and physical science.

On the other hand, as Singer (E.B 15,201) and Foster (*op. cit.*, p 151) agree, in thinking that all problems of life can be completely interpreted in terms of chemistry, Sylvius went too far. His iatro-chemistry, like the iatro-physics of Borelli and the vitalism of Stahl and others in this century, represented one approach to the understanding of living things, but not the only one

Still, it was significant that mechanist views could now find support from the elementary science of chemistry as from mechanics, especially among those not given to the study of organisms as living wholes. The study of the chemistry and mechanics of isolated tissues and organs has indeed very frequently lent itself to mechanistic interpretation (*cf.* II(*b*), i and ii; III(*b*) 3, ii, below). If a part is abstracted from a whole it is not unlikely that its functions will be found to be quite differently conducted from those of the whole itself.

(ii) *Descriptive and systematic biology.*

During this period the number of plants and animals known to science became vastly increased. Explorations of little known countries were made; discoveries of new lands, notably of Australia and many islands, introduced new types of animals altogether; intensive study by brilliant naturalists revealed many hitherto unnoticed species even in such countries as England Ray, Willughby and Gilbert White in Britain, Belon and Rondelet in the Mediterranean region, Linnaeus in Scandinavia, and Cook and Banks in Australia,

may be noted as students of animals, while numerous botanists multiplied the number of plant species noted by C Gesner and the German botanists of the sixteenth century. These huge increases in animals and plants called for new methods of classification, such as those of Ray and Linnaeus and the French botanists

Much of the natural history of this epoch was concerned with collecting and classifying new forms rather than with observation of living beings in their habitat, though this was done too. The total effect upon vitalistic theory was not strongly marked, but there was an effect and it supported vitalistic beliefs. Whatever chemists and physicists might make of laboratory experiments and pathological observations, the old distinction between living and non-living things seemed sound; animals were animals and plants plants all the world over; they developed, lived and reproduced as organisms and behaved quite differently from inanimate objects. This extended knowledge did good service too in banishing mediaeval beliefs in monsters like the roc, the phoenix, and such like Though many new organisms were discovered, they differed from more familiar ones in degree, not in kind. The ordinary laws of physics and chemistry, as far as they were known, were valid in the world of life, even if other laws were valid too.

(iii) *Philosophy; Leibniz; Kant, etc*

Descartes, a great philosopher and a poor biologist, conceived of man in a dualism which became a basis for later mechanism. Newton's mighty influence tended to be used in the same direction, as we have seen. He was not a biologist but a believer in mechanics and in God as revealed in Christianity and as the necessary First Cause—a similar position to that of Descartes.

Leibniz (1646-1716), too, believed in God (*cf.* Wolff, *History of Science*, pp. 630, 663, 672) and in mathematics, but he developed a system of philosophy which influenced biology as well as other branches of knowledge. In his bachelor's dissertation he supported the nominalist opinion that "individuality is constituted by the whole entity or essence of a thing" (E.B. 14, 884), which is vitalistic; and

more generally than Galen, he taught that, on the whole, all was for the best in the best of all possible worlds His peculiar philosophical system—monadology—might seem to have no particular bearing on biological theory But, despite his personal beliefs, Leibniz' philosophy, like that of other great philosophers of the period, was largely mechanistic For him, as for Newton and Descartes, Nature was "a mechanical system, arranged by GOD" They distinguished, like many after them, the spiritual sphere from "nature", so in a metaphysical sense they were anti-materialists: and Leibniz particularly is at great pains to establish clearly the relation between these two great divisions of Being But as far as nature itself, life included, is concerned, Descartes and Leibniz and their followers behaved like materialists" (Driesch, *History of Vitalism*, pp 23-4)

This mechanistic trend in philosophy was strongly developed in the eighteenth century by Laplace, Legrande and others, and was popularised by such writers as Voltaire. It was stoutly applied to man, and inferentially to biology in general, by de la Mettrie, French physician to Frederick the Great, in his book *Man a Machine* (1748), which was replied to by an anonymous author in *Man not a Machine.* De la Mettrie seems to have been the first author in Christendom to adopt a purely mechanistic view of man's being; but "by attacking Christian morality as well as theism he incurred widespread reprobation" (Whetham, *History*, p. 215; *cf* J Needham, pp 114-15)

P J G Cabanis (1757-1808), also a Frenchman, is an admirable example of the conflict between mechanism and vitalism during his period A supporter of the French revolution, he first enunciated the view, adopted by Vogt and others a hundred years later and still current in certain circles to-day, that "the brain produces thought" as the stomach, liver and other glandular organs produce their typical secretions, "and the sub-maxillary gland secretes the saliva" (J Needham, p. 115; for refutation of this fallacy see III(*b*) 2, i and iii). Yet, though he first adopted the purely materialistic views by which alone he is generally known, Cabanis eventually "went over to the vitalistic school of G. E. Stahl" (E.B. 4, 495c; 18, 711a).

E. Kant (German, 1724-1804), like many other great men, changed his opinions, or at least his expressions of them, considerably from time to time It suffices here to note Driesch's careful summary of Kant's relation to vitalism. Though "he leaves much undecided and writes very indefinitely" he is "very far from being a dogmatic 'parallelist', or . . . a phenomenalistic materialist" He speaks of the dust of which man is composed "as an animal creature" returning to dust "after having been endowed for a short time with the force of life" (*History of Vitalism*, pp 91-2) So Driesch believes that Kant can be considered as "in the case of man and his actions . . indubitably a vitalist, while as regards the facts of organisation he is only problematically so" (*op. cit*, p 86)

It is well to bear in mind, as Driesch points out (*History of Vitalism*, p. 25), that though in the philosophy of the seventeenth and eighteenth centuries "the whole theory of nature is under the influence of mechanics" so that the theory of life was, or tended to become, mechanical too, nevertheless Descartes and Leibniz, like Newton and Locke, were not "metaphysical materialist." For them "the things which are seen [though interesting, even wonderful] are temporal; but the things which are not seen are eternal" (2 Cor. 4, 18). To such stout believers in a spiritual world, who devoted much time and ability to religious as well as to scientific and philosophic matters, there was no *a priori* objection to vitalism or to the possibility of supernatural intervention in mundane affairs. The latter indeed was part of Descartes' philosophy. They were not biologists and so were not immediately concerned with the possible existence of vital forces; their philosophy rightly suggested that mechanical explanations should be found for the phenomena of life as far as possible; but it was not essentially opposed to such ideas as Stahl's sensitive soul or to an "essential life force" indicated by the growth of knowledge of embryology.

(iv) *Embryology; the classical microscopists; pre-formation; epigenesis; C. F. Wolff; Haller.*

From Harvey onward there is a continuous stream of workers who specialise in one of the fundamental studies of

organisms, that of their development. Their views are of great importance, one reason being that they are compelled, at least in the early stages of becoming, to consider the organism as a whole.

Harvey's great work on embryology was soon surpassed by that of Malpighi (1628-1694) as regards observation, though "Harvey's work is more philosophical" (Locy, p 197) "The first adequate description" of the development of the chick, copiously illustrated by excellent drawings, was given by Malpighi, "and where he left it, so for the most part the matter remained until even the nineteenth century" (Foster, p 92)

Malpighi was one of the great pioneers of microscopy, the others being Hooke (1635-1703), Grew (1628-1711), Swammerdam (1637-1680), and Leeuwenhoek (1632-1723); their work was continued in the eighteenth century by Lyonet (1707-1789) and others (*cf* Locy, Chapters IV and V). Their discoveries had a great influence upon the development of biological theory

Malpighi (1661) and Leeuwenhoek more completely in 1686 observed the capillaries and the blood coursing through them, thereby completing the proof of circulation of the blood which Harvey had foreseen This finished the demonstration of the erroneous teaching of Galen and so of the crude vitalistic theory based upon it But the destruction of the false is an essential step in the discovery of the true.

They vastly extended the scope of biology; the Protista were discovered and described; Leeuwenhoek even succeeded in observing bacteria, the amazing complexity of the intimate structure of large organisms was described in numerous beautiful monographs and "anatomies"; Swammerdam first saw and described blood corpuscles, Hooke the cellular structure of plants. Spermatozoa were discovered by Leeuwenhoek and his student, Hamm, about 1677, though Hartsoeker claimed priority. Knowledge was increased and the need for a corresponding development in theory made apparent. The unquestioned belief in spontaneous generation was first challenged by Hooke, Swammerdam, and Redi (see II(*c*) below).

Embryology passed from a macroscopic to a microscopic

stage, and so the way was opened for the great "evolution"-epigenesis controversy of the eighteenth century with its important bearing upon vitalism.

"Evolution" in this connection means the unfolding or development in the embryo of structures already present in the generative element or cells. But to avoid confusion with theories of descent the term pre-formation will be used for this theory. It is comparable to the growth of a twig from the bud of a plant, whereas epigenesis, the development of quite new structures, not present as such in the ovum or spermatozoon, is analogous to the original formation of adventitious buds on plants Some believers in this evolution or pre-formation held that the adult form was present in miniature in the sperm; these are known as "animalculists," and some of them published sketches of the sperm as a homunculus with a flagellum Among them were Leeuwenhoek, Hartsoeker and Leibniz

Swammerdam and Malpighi, followed by Bonnet and Haller, were "ovulists"; they believed that the ovum contained the adult in miniature, or at least representations of the tissues and organs of the adult In such cases, *e g.*, Bonnet and Leibniz, the theory of *emboitement* followed logically. If the adult is pre-formed in the germ, its generative organs are pre-formed with the others, so are its germ cells, with the next generation already pre-formed in them; and so on *ad infinitum* This view implies the fixity of species and the special creation of each, not of its first parents only, but of all its generations once and for all (*cf.* Locy, pp. 207-11, and Driesch, *History of Vitalism*, pp 38-44)

Epigenesis means the gradual development from the undifferentiated ovum of structures not present in it in any material way; though the indisputable facts of heredity show that in some way each germ possesses the power of producing certain definite tissues and organs. First advocated by Harvey (*q.v.*), this theory was eclipsed for a time by pre-formation; but in the eighteenth century it was advocated by Needham, Maupertius and others.

Buffon believed in epigenesis for the germ and in pre-formation for its development; but in order to avoid the obvious difficulties of *emboitement*, he had to develop a peculiar

theory rather similar to that of Darwin's now quite discredited pangenesis Because of this Driesch claims him as a vitalist (*op cit*, pp 40-43), but the particles which Buffon's "*moule intime*" or vital force arranged in the genital organs are so unlike anything known to science that a bare mention of his theory is sufficient Anyhow, it fails, like the *emboitement* form of pre-formation, before observation with high-powered microscopes.

T. Needham's theory is bound up with belief in abiogenesis (see II(*c*) below). Maupertuis, who was a mathematician, followed Buffon in his theories about embryology. But the first chief exponent of epigenesis after Harvey is C F. or F K. Wolff,[1] the publication of whose *Theoria Generationis* in 1759 marks an epoch in the history of biology. His *De formatione Intestinorum* developed the germ-layer theory in more detail. Although opposed at the time by Haller, the great physiologist, and by Bonnet, the great zoologist, Wolff's ideas were developed later by Von Baer, Meckel, F. M. Balfour, and others to form part of the basis of modern embryological work. C F. Wolff stood for gradual differentiation of parts from the undifferentiated germinal elements (knowledge of cell nuclei and protoplasm, etc, was not acquired till the nineteenth century); so gradually the preformation theory, with its *emboitement* corollary, disappeared.

Yet Locy considers Wolff's "theory of development was entirely mystical and unsatisfactory" (*op cit*, p 210). While opposing *emboitement* or the physical theory of inheritance he was as ignorant as others of his time of the continuity of germinal substance (Locy, pp. 224-5) and of the "inherited organisation of great complexity" with which "the egg and the sperm are endowed"

Locy's criticism is too severe; yet Wolff's *vis essentialis corporis*, though a striking idea in the history of vitalism, is a crude conception compared with the entelechy of Driesch, for it was supposed to act on inert germinal material and practically create the new organism. Driesch (*History of Vitalism*, pp 46-9) is, however, highly appreciative:

[1] Wolff's Christian names are given as "Caspar Friedrich" by Driesch (*History of Vitalism*, p 44) and Sachs (Book II), but as "Friedrich Kaspar" by Locy and others. This confusion of names and initials is regrettable.

The *vis essentialis* directs the passage of nutriment from the egg into the embryo when it has no heart or arteries and "pre-formed canals" are not visible. "It directs the epigenesis as later it will direct the conservation of the mature body." There are indications in Wolff's writings of a quite modern outlook; he sees that his "essential force" can unite with the agents of the inorganic, and he does not claim to "explain" anything, only to have "investigated the connection which exists between machine and life," but not "the causes of the latter where it has no relation to machine." The developing substance works "as far as it is endowed with certain qualities" *not* "as far as it is constructed in a certain way." Thus Vitalism, or what Driesch terms dynamic teleology, is clearly expressed and static teleology "expressly rejected" So Driesch finds Wolff "the cleverest and deepest representative of Vitalism since Aristotle."

Driesch claims that "all believers in epigenesis are vitalists" (*History of Vitalism*, p 39), and appears to suggest, implicitly at least, that pre-formationists are not (*ibid*, pp. 49, 53). A critical examination of this position may well be made here.

The first point to notice is that, whatever explanatory theory was adopted, the embryology of animals was seriously studied by Harvey, Malpighi, Wolff and their contemporaries for the first time since Aristotle's *De generatione* two thousand years before Little was known about the development of plants till the great German botanists arose in the nineteenth century (see Sachs' *History of Botany*).

To the physiology and philosophy upon which Descartes, Stahl and others based their biological theories a new foundation for such theorising was thus added. The tissues of adult animals, including Man, were recognised as arising in some fashion from a very small embryo, in the first place an ovum, in which they were originally either non-existent (epigenesis) or existent in a different and greatly reduced form (pre-formation). But—somehow or other—from the ovum of the hen a chick developed; in the human uterus a baby appeared; and so on It was quickly recognised that nothing like this was known in inorganic nature, though analogies were drawn between the growth of animals and that of dendritic forms of inanimate matter, notably by

Maupertuis in his *Venus physique* (1746), but Maupertuis held the monistic idea that inorganic particles possess some of the properties of living matter (*From the Greeks to Darwin*, p. 113; Driesch, *op cit*, p. 143)

Harvey (*q v*), Wolff, von Baer (*cf* II(*d*), v), and other exponents of epigenesis taught that some "*vis essentialis*" or similar power, unknown in inorganic nature, is required to account for the marvel of animal development. So Driesch's claim that all believers in epigenesis are vitalists may be accepted as substantially correct for this period, though phrased in too sweeping a form And the criticisms by Driesch and Durken of recent mechanistic views of embryology, given in III(*a*), ii and III(*b*), 3, v, show, in my opinion, that all epigeneticists should be vitalists

The pre-formation theory is intimately connected with two beliefs held firmly in the seventeenth and eighteenth centuries, the fixity of species and the special creation of each, or, at least, as Linnaeus (1707-1778) held latterly, the special creation of a genetic type from which allied species might be descended A writer in the first decade of the twentieth century might well consider that *non tali auxilio* should the case of vitalism be propounded, and, of course, the whole idea of *emboitement* is not only obsolete but alien to the knowledge of embryology on which Driesch's own idea of vitalism is based (*cf* Part III(*a*) below), for he claims in his theoretical analysis that "it is with unifying becoming alone that Vitalism has to do" (*op cit*, p 214), and describes the views of Bonnet and Haller as "anything but Vitalism" (*op. cit*, pp 53, 57).

Later Driesch recognises that epigenesis represents some truth, but not quite the whole. There actually is a certain amount of pre-formation in the "intimate organisation of the protoplasm" of the egg, though it is not "strict" evolutio (*Science and Philosophy of the Organism*, p. 48).

But a vitalistic outlook, in a general sense, appears to me essentially involved in the "evolutio" theory of ontogeny, even though masked by the two beliefs mentioned. The creation not of one but of all successive generations of a species obviously implies the presence of directive agency, force or—in the Aristotelian sense—entelechy which will

secure the due development of each adult, though this is not so evident as it is on the hypothesis of epigenesis.

Thus, the new knowledge of animal development, whatever theory it was linked with, reinforced the vitalistic belief which was threatened by mechanistic ideas in physiology and cosmology. Only, any beliefs connected with pre-formation were destined to collapse as that astounding theory died out; whereas the epigenetic concept of development continued to be an actual or potential support for vitalism despite the mechanistic conceptions which arose in the nineteenth century (*cf.* II(*b*) and II(*d*), v).

*A. von Haller (Swiss: 1708-1777).*

The view that pre-formation involves vitalism, though of a type that was bound to die out, is well illustrated by consideration of one who in eighteenth-century physiology occupied a place analogous to that of Linnaeus in classification. Haller's writings were most varied and voluminous, and his *Elementa physiologiae corporis humani* (1757-1766) is a landmark in the history of this branch of science. "His conceptions of the nature of living substances and the action of the nervous system . . are still integral parts of physiological teaching" (Singer, *History of Biology*, p. 367). He emphasised irritability as a most important property of living animal substance, by which a slight stimulus may produce a great movement He rejected the views of Descartes and Stahl about the seat of the soul and located it in the medulla of the brain, where, he considered, sensation and movement have their source (Singer, *loc. cit.*; Foster, Lecture X).

Though he started as an epigeneticist he soon adopted the opinions of Swammerdam and Bonnet and became the chief exponent of pre-formationism and the opponent of C. F. Wolff. "God has created all structures; they do not develop but only grow; . . . 'omnes simul creatae existunt'" (Driesch, *History of Vitalism*, pp. 53-7). This indicates the extremely close connection between pre-formation ideas and belief in the direct creation of species (*cf.* II(*a*), i).

The difficulty of summarising Haller's position is shown by the divergent accounts given by historians of biology. Locy (*Biology and its Makers*, pp. 181-2) follows Verworn in

thinking that misinterpretation of Haller's work on irritability led to a conception of "vital force" as a "mystical supernatural agency" which retarded progress for a long time. Singer says that "he develops no mystical or obscure themes" (*loc cit*, p 258). Driesch (*loc cit*) notes that Haller rejected the machine theory of organisms of de la Mettrie and others, but criticises his views as "a machine theory of a static teleology—of anything but Vitalism"; though earlier he complains of the suppression of Haller's vitalistic views and of his rejection of the machine theory! And he finds obscure the suggestion that "autonomous forces" are implied in Haller's doctrines of irritability and contractibility.

Verworn (*Irritability*, pp. 2-5) pays tribute to Glisson (English: 1597-1677) as the founder of the doctrine that irritability, in varying degrees, is "a general property of all living substance"; but, as he admits, Glisson's obscurely developed ideas had hardly any effect upon scientific thought.

CHAPTER 3

# THE REVIVAL OF VITALISM: BICHAT: BLUMENBACH

THE notable change that occurred at the end of the eighteenth century in the philosophical and biological outlook is well summarised by Whetham. He attributes the seventeenth-century "naturalistic turn" in physiological enquiry to Harvey, and, with Mach, "the general wave of heretical thought" to the "mechanical philosophy" based on "the astonishing success of the Newtonian theory." "But in the second half of the eighteenth century the difficulty of the (physiological) problem led to the almost universal adoption of the hypothesis of vitalism . . . which retained its influence till the middle of the nineteenth century" (*History of Science*, pp. 279, 213).

Driesch speaks of the epoch of Bichat and Blumenbach as "the end of the second period of the old Vitalism," and also as its "height" (*History of Vitalism*, p. 64). J. Needham, too, takes the view that "the eighteenth century closed with the general acceptance of a mild vitalism, represented by Bichat and J Muller" (though Muller was not born till 1801!), adding, reasonably, that "it was the calm before the storm" (*Sceptical Biologist*, p. 115). A revival of vitalism at the end of this period is therefore generally accepted.

Needham finds a partial explanation in the rise of the romantic movement in thought and literature (*op. cit.*, p. 118). As regards England the evangelical revival played a part in counteracting an unduly materialistic view of life, and so, no doubt, affected the general outlook on science. The great change effected in the nation by this cause is well illustrated in two books of a very different nature—*Lives of the Rakes*, especially Vol IV, *The Hell Fire Club*, and the Rev. G. R. Balleine's *History of the Evangelical Party in the Church of England*. The former shows how many influential persons

were indifferent or even hostile to Christianity in the early and middle parts of the century; the latter describes also the change that gradually occurred[1] In France, the revolutionary abolition of religion was ephemeral and the Roman Catholic Church largely regained its influence In both countries increased reverence for the Bible resulted. In addition, after the excesses of the French Revolution, which involved the death of Lavoisier and other scientists as well as of many other persons, there was a general anti-revolutionary movement in Europe in politics and thought (Trevelyan, *British History in the Nineteenth Century*, pp. 52-4, 60-80; Marriott, *Evolution of Modern Europe*, Ch. XXIV). This tended to discredit the materialism characteristic of many Frenchmen of the pre-revolutionary period, such as Voltaire and Laplace; and, as we have seen, Cabanis changed his biological philosophy completely.

Nor was this general trend of thought unaccompanied by definite support from biology The rigid mechanism of de la Mettrie and others was found inadequate as a biological theory (*cf* Ch. 2, iii, above). Epigenesis, as elaborated by C. F. Wolff, became better known, and this strengthened belief in vitalism (*cf* 2, iv, above).

So did the new branch of biology—histology or the study of tissues M. F. X. Bichat (French 1771-1801), author of *Recherches Physiologiques sur la Vie et la Mort* and other works, and collaborator with Cuvier, is generally regarded as its founder (Locy, pp. 166-70). "He held that there is in life a conflict between vital forces and those of physics and chemistry, which after death resume their undivided sway" (Whetham, p. 274). His influence was great and he supported vitalism, claiming that his *propriétés vitales* were similar to gravity, elasticity, and other recognised physical forces. Driesch criticises his outlook and methods severely, largely because he made no use of the study of morphogenesis (*History of Vitalism*, pp. 60-62). But at a critical period he was an important exponent of the principle of the autonomy of the organism.

This may also be said of the famous anatomist John

[1] *Cf. England before and after Wesley*, J. Wesley Bready, Hodder & Stoughton, 1938.

Hunter (English· 1728-1793) He thought that a "latent heat of life" existed, and regarded life as the cause, not the consequence of organisation In this he was followed by Treviranus, Huxley, and others (*cf* Singer, pp. 208-12, 369-70; Wallace, *World of Life*, p 284).

Though J F Blumenbach (1752-1840) lived far into the nineteenth century, his short but important contributions to biological theory (*Institutiones physiologicae* and *Ueber den Bildungstrieb*) appeared in 1787 and 1789 He enumerates the "vital forces" of Haller—contractability, irritability and sensibility—together with a *vita propria* or special vital activity of the organs; though, like Haller, he is uncertain whether these indicate an "autonomy of vital activities" or not. But in the second book he describes how he abandoned pre-formation in opposition to Haller, and elaborates his own conception of a "nisus formativus." This is a power peculiar to living bodies though it is only one among the "vital forces" mentioned above He compares it with gravity· both are recognisable through the effects they produce; but the real cause is a hidden quality like those of other physical forces. Much of his argument proves epigenesis rather than vitalism. But he argues well for his "formative impulse" from the phenomena of restitution in *Hydra* and the healing of large wounds

Driesch ends what he terms the second period of the old Vitalism, which he commences with Harvey and Stahl, with high praise of C Fr. Wolff, and "above all" Blumenbach. "Only now," he writes, "do we get beyond the achievements of Aristotle" (*History of Vitalism*, pp 57-65). But it is hard to see why Blumenbach is singled out for such high praise except for his argument from the restitution of parts. He was a great anthropologist, but Wolff first advocated epigenesis against the prevailing opinion in favour of preformation and reinforced his arguments by first-hand study of the phenomena of development.

C. Fr Wolff, the pioneer in embryology, is the man who, with greater knowledge than was available in Harvey's time, clearly indicated its importance for the view that organisms exhibit the action not only of ordinary physical forces but of something more. He is to be looked upon as opening a

new epoch in the history of vitalism or, alternatively, as terminating an earlier one.

There is one other dictum of Driesch regarding this period which invites comment and general agreement. During the period from Harvey to Wolff and Blumenbach, "biology, which used to be an appendage of philosophy . . . changes into a science, clearly and firmly fixed." Foster and Locy place Vesalius (1516-1564) as the first to challenge authority and base biology on an observational basis; but Harvey is the first great experimentalist So the spirit, if not the letter, of Aristotle was revived. Vitalist and mechanist both became free to observe, propound and criticise.

# I (*c*).—SUMMARY OF PART I

"Without hypothesis there can be no progress in knowledge."
M VERWORN (*Irritability*, p. 259).

IN the period from antiquity to the close of the eighteenth century the following phases of vitalistic thought are distinguishable.

(i) The first clear vitalistic theory to be based on scientific study is that of Aristotle (I (*a*), i). Its main features are the idea of entelechy or "form" as an autonomous power in living things and those of the three "souls"—vegetative, sensitive, and rational, characteristic of plants, animals and Man respectively. These ideas have been revived again and again by later biologists, and, modified, form an important part of modern vitalism (III(*d*), 2, v).

(ii) The doctrine of natural, vital, and animal "spirits", though partly held by Erasistratus (I (*a*), ii), is due chiefly to Galen (I (*a*), iii). It is based on ignorance of anatomy and physiology, and largely collapsed after Harvey established the circulation of the blood But the conception of special (animal) "spirits" elaborated in the brain, though most erroneous in details, is not to be dismissed with contempt. It was not immediately overthrown by Harvey, as is sometimes stated, and was a forerunner of later, more refined, ideas of the pre-eminence of the brain and the nervous system in the life of the higher animals. In this respect Galen's physiology was an advance upon that of Aristotle. Otherwise, the "vital fluid" idea was a mistaken one, a form of vitalism which was bound to fail before the advance of science after the Renaissance. Galen did not use embryology or psychology to support his vitalistic theory as did Aristotle.

(iii) In addition to destroying the basis for Galen's wrong ideas Harvey (I (*a*), iv) revived and extended some of Aristotle's teaching; he taught epigenesis and that as due to an internal vital force or "opifex"—"a peculiar vital auto-

nomy" (Driesch). Harvey therefore occupies an important positive position in the history of vitalistic theories.

(iv) Descartes' influence on biological philosophy has been strongly mechanistic (I(*b*), 1, i) His ideas about animals and plants were mechanistic, only for Man was his theory vitalistic, to a strictly limited extent; and this depended on fanciful and false notions about the seat of the soul in the pineal gland and a continuance of Galen's animal spirits in the brain These notions were soon exploded, and so the slight vitalistic part of Descartes' doctrine was destroyed.

(v) Following Paracelsus, van Helmont (I(*b*), 1, ii) elaborated a vitalistic counterblast to the mechanism of Descartes According to his doctrine of "blas", all bodily functions are actuated by a complicated spiritual hierarchy. But this theory, too, soon passed Arguments for the independent existence of the human soul or spirit do not apply to "blas" and archaei; there is no evidence for their existence and much against it; and theoretically, too, on the principle of Occam's razor, they must be discarded. Again, his placing of a double soul near the pylorus is an error which helped to discredit his whole system

(vi) The most important development since Aristotle of a biological theory of the soul is that of Stahl (I(*b*), 1, iii). It is free from the idiosyncrasies of van Helmont, though marred by the postulate that chemical changes in the body are essentially different from those witnessed in the laboratory. But he revived Aristotle's distinction between living and non-living things as *the* important division for biologists, and anticipated Bergson and other moderns in emphasising the independence of the soul or spirit as a "sensitive soul": this, he taught, controlled all changes in the body. Though he ignored what was known of development at his period and so had no equivalent to Driesch's two equipotentialities, he also used animal movements together with the phenomena displayed by plants, but only in a secondary and superficial manner, as a basis for the vitalism which he maintained as a corrective to the iatro-physics and iatro-chemistry of his period.

During most of the eighteenth century the trend of phil-

osophy was on the whole anti-vitalistic. The great achievements of Newton, Leibniz, and others had indicated that nature was governed by mechanical laws True, like Descartes and Locke, they were not "metaphysical materialist" only scientifically materialist; but other thinkers, such as Laplace and Hume, were "metaphysical materialist"; and biology gradually became affected by the general character of the period Vitalism, as far as it was touched upon by the philosophers, was conceived of mainly or even wholly as an attribute of the "rational soul" of man (*cf.* I(*b*), 2, i and iii), as is illustrated by the varying ideas of Kant

But towards the end of the century vitalism became the prevalent belief (*cf.* I(*b*), 3), and the change is well illustrated in Cabanis (I(*b*), 2, iii) This change was due to many factors, some unconnected with science, and the general vitalistic outlook produced was largely uncritical It was, however, supported by several biological theories —

(vii) Epigenesis (I(*b*), 2, iv), advocated a century before by Harvey, was revived by C F. Wolff, and this important view of morphogenesis gradually spread despite the powerful opposition of Bonnet and Haller As Driesch has shown, this itself is an argument for vitalism Wolff combined with epigenesis the particular vitalistic explanation of a *vis essentialis* to account for development in animals generally, so that vitalism ceased to be connected with the peculiar mental powers of Man only

(viii) As I have shown above (I(*b*), 2, iv); *cf.* II(*a*), 1, below) even the pre-formation doctrine of Haller and Bonnet, though opposed to epigenesis and incorrect in fact, also had a vitalistic effect, even though Driesch is correct in finding it only "static teleological" (*op. cit*, p. 49). The fact is that study of development tends to develop vitalistic beliefs, whatever particular idea it may be associated with. In addition, "evolutio" was closely connected with belief in the special creation of living beings, which itself suggests that such beings possess special powers (*cf.* II(*a*), 1, below).

(ix) Irritability as "a general property of all living substance" is an important vitalistic doctrine originated by Glisson, but developed and popularised by Haller (I(*b*), 2,

iv). It is still accepted by biologists, including such mechanistic writers as Semon and Verworn.

(x) "Vital force" or "forces" This belief, though varying in different writers and often ill-defined, became generally accepted Unlike the *vis essentialis* of Wolff, it was based not only upon epigenesis (Blumenbach) but on general physiology (Haller) and notably on histology, newly founded by Bichat (I(*b*), 6; *cf.* Part II(*a*), *passim*, below).

(xi) Redi and Spallanzani had destroyed crude ideas and superstitions about spontaneous generation, and, as far as it went, their work strengthened true vitalism enormously, for it made absolute, at the time, the formerly ill-defined division between living and non-living things. But abiogenesis was not yet dead, and was combined with a poor type of vitalism by the nature philosophers (*vide* II(*a*) below; *cf.* II(*c*)).

# PART II

## THE NINETEENTH CENTURY, TO 1895. VITALISTIC BELIEFS AND CONFLICTING DISCOVERIES AND IDEAS

# II(*a*).—VITALISM IN THE FIRST HALF-CENTURY

"O Lord, how manifold are thy works! in wisdom hast thou made them all: the earth is full of thy riches"

PSALM CIV. 24.

(i) *Its general acceptance Special creation of existing organisms, endowed with special powers, generally believed till 1859. "Vital force" ideas*

DRIESCH rightly considers this half-century the last period of "the old Vitalism . . . an age when everyone thought vitalistically" (*History of Vitalism*, p. 113) It continued the movement begun towards the close of the eighteenth century described above in I(*b*), 3.

Here we may note more specially the strength of belief in the fixity of species as specially created "in the beginning." In the eighteenth century, when belief in cosmic evolution was potent under the leadership of, among others, Laplace and Kant (*General History of Nature and Theory of the Heavens*, 1755), it was natural that suggestions should be put forward about a corresponding development among living beings, and this was done to a limited extent by Linnaeus (1707-1778) and Buffon (1707-1788) Linnaeus used the expression *nullae speciae novae* in his early writings, but later adumbrated the view that existing species were derived from those first created by "hybrid generation", all the species in one genus being derived from one original species (Osborn, pp 128-30; Sachs, Book I). Buffon expressed "transmutationist" views between 1761 and 1766, but returned later to the idea that species are more or less fixed (*cf.* Osborn, pp. 130-39). Buffon was, however, far from clear, and the great influence of the encyclopaedic Linné was practically entirely on the side of the fixity of species.

During the early years of the nineteenth century the well-known evolutionary works of Erasmus Darwin (1731-1802), Lamarck (1744-1829), and Treviranus (1776-1837) appeared,

followed in 1844 by the *Vestiges of Creation*, attributed to Robert Chambers. These, with other minor precursors of Darwin and Wallace, are well described in *From the Greeks to Darwin* (Chs. IV-VI) and in the lives of Darwin, Huxley and others All we need note here is the inability of any of these writers to influence their contemporaries

Referring to the time when he thought that "the evidence in favour of transmutation was wholly insufficient" Huxley stated, "within the ranks of biologists, at that time, I met with nobody except Dr Grant . . who had a word to say for evolution . and his advocacy was not calculated to advance the cause. Outside these ranks, the only person known to me whose knowledge and capacity compelled respect, and who was, at the same time, a thorough-going evolutionist, was Mr Herbert Spencer" (*Life and Letters of C. Darwin*, ii, p 188, *cf* II(*b*), iv, below) Darwin himself has written· "I formerly spoke to very many naturalists on the subject of Evolution, and never once met with any sympathetic agreement It is probable that some did then believe in Evolution, but they were either silent, or expressed themselves so ambiguously, that it was not easy to understand their meaning" (Osborn, p. 227) Haeckel bears the same witness· Lamarck's theory of evolution "was overlooked for half a century . . it only obtained general recognition when Darwin had supplemented it . . by the theory of selection in 1859" (*Wonders of Life*, p. 378).

The defeat of these early attempts to apply evolutionary ideas to organisms was largely due to the great knowledge and influence of Cuvier (1769-1832), who crushed Lamarck and G. St Hilaire (1772-1844) by reference to undoubted facts in opposition to what he showed were theories Osborn points out that "St. Hilaire's method was professedly inductive" but that he was not consistent with his profession (*op. cit.*, p. 197); and the same criticism can be passed upon other exponents of transformism, unless, like Oken and the great Goethe, they were not even inductive in theory. Cuvier's influence extended far and long (*cf.* Osborn, pp. 196, 204; and III(*c*), 2, i, below).

This demonstration of the strength of the belief in the special creation and fixity of species is given here because

of its immediate bearing upon the history of vitalism. It emphasised the distinction between organic and inorganic involved in the chemical idea of "vital force," and agreed with the implication of epigenesis that an organism was something unlike anything found in inorganic nature.

And, in spite of the general modern belief in organic evolution of some kind or another, there is no reason to suppose that the ideas of Cuvier and his colleagues were all entirely wrong. Huxley only adopted Darwinism "subject to the production of proof that physiological species may be produced by selective breeding" (*Man's Place in Nature*, p. 100). Modern species at least are very constant (*cf.* *Coming of Man*, p. 22); and there is good ground for believing not only in the special creation of life but even of that of numerous kinds of organisms (*cf.* II(*c*) and III(*b*), 3, iii, Berg), which is not a very different notion from Cuvier's often derided idea of waves of creation following catastrophic changes in bygone ages While, before the modern arguments for the evolution of species from geology, geographical distribution, and comparative embryology had been developed, the reasons for believing in the at least comparative fixity of species were very strong. In considering this aspect of biological thought it is usual to compare two doctrines—the fixity and unfixity of species—to the disadvantage of the former. It is also to be noted that the special creation idea was in conflict with another theory, that of abiogenesis; and that the latter is certainly wrong, so far as we know, and has led to much erroneous theorising, vitalistic and mechanistic (*cf.* II(*c*)).

The epigenetic theory of animal development gained ground, in spite of Cuvier's unreasonable opposition, notably through Meckel and von Baer But as Driesch points out (*History of Vitalism*, p. 101), morphogenesis occupied a secondary place, and the study of wild animals was still rudimentary.

The outstanding vitalistic idea of the period was that of "vital force." It originated earlier; for instance, apart from Stahl, P. J. Barthez, a famous French physician in his day (1734-1806), in his *Nouveaux élémens de la science de L'homme*, used "vital principle" "as a convenient term for the cause of

the phenomena of life" (E B, 3, 152b). But the conception was typical of the early part of the nineteenth century, though it was not always maintained in the same way. In its extreme form it implied that living things possessed the power of creating energy (*cf* Liebig, *Animal Chemistry*, pp. 29, 31); in a moderate form it taught that vital force was similar to gravity and electricity (*cf* Liebig, *passim*), a doctrine which has been revived on a small scale by Ostwald (*Science and Philosophy of the Organism*, p 256) In either fashion it was, of course, described as a peculiar property of living things and particularly as a property relating to chemical change. "Organic" compounds were compounds built up by organisms, and were believed to be unobtainable otherwise. Urea was a typical example (*cf* II(*b*), 1) The connection of this conception with that of the special creation of living creatures is obvious Organic species were created with special forms and special powers; of these reproduction was one, a vital force able to make compounds useful or essential for life was another The growth of plant physiology favoured this belief. For instance, the general nature of photosynthesis was worked out by Ingen-Houss (1730-1799), Senebier (1742-1809), and T. de Saussure (1767-1845) in 1779, 1800, and 1804 (Sachs, Book III, Ch II), and it was quickly and rightly perceived that nothing like the transformations achieved by green leaves in sunlight at ordinary temperatures could be accomplished by the best chemists with the best apparatus And until Wohler, C. Bernard, and others had made their crucial experiments, what was known of the intricacies of animal physiology agreed with the vital force idea too (*cf.* section iii below).

It is now desirable to epitomise the vitalistic teaching of representative thinkers of the period, leaving its anti-vitalistic discoveries and opinions to the next sub-Part. Vitalistic thought may be considered under two main heads—the nature philosophers, with whom may be grouped what Driesch terms the "dogmatic school", and the chemical school.

(ii) *The Nature Philosophers: Oken, Treviranus, etc.*

According to Driesch (*History of Vitalism*, I (*e*)) the philosophy of Schelling (1775-1854) offered exposition, not

explanation; and, in its general aspect, "the theory is irrelevant for biology, especially for Vitalism" The conception of "type," too, is unimportant here, so we are not concerned with the views of Goethe. Nature-philosophy enters into relation with the problem of vitalism "when we try to connect the world of ideas with the world of direct data"

L. Oken (German: 1779-1851) is the outstanding writer of this school. In his famous work on nature-philosophy, he attributes the principle of life to "galvanism"; his references to vital force are not clear, and are negative rather than positive. The book is full of absurdities, of dogmatic guesses about origins Driesch considers his *Die Zeugung* somewhat better, although Oken adheres firmly to abiogenesis in spite of the experimental work of Spallanzani and others. Absurdities abound in this book too For instance, as Driesch quotes (*ibid*, p 97), "generation . is the synthesis of the infusoria by means of the homogeneous but opposite pole of the organic world ... The feminine vesicle provides—only the form which unites the entering cercariae. . . " And so on

A more critical view is that of H F Osborn, who has no patience with the lavish praise given by Haeckel to Oken; nor would he agree with Driesch's description of him as a "great anatomist" He shows that Oken's conclusions, such as they were, were based upon *a priori* premises, and his biological ideas upon metaphysical conceptions Thus, the "All" was connected with the sphere, and the skull considered a manifestation of this "archetype"; so, too, the vesicles of the *Ur-schleim* Through abiogenesis the "infusoria" arose; all higher animals and plants consist of these; "the animal is the highest union of polyp and plant, of line and circle (!)" (Driesch, *ibid*, pp. 97-8) "All life is from the sea; the whole sea is alive. Love arose out of sea-foam." Man "probably arose in India . . . the offspring of some warm and gentle sea-shore", owing to "a certain mingling of water, of blood warmth and of atmosphere." Yet, again: "God . . . took . . . an earth-clod or carbon; moulded it into form, thus making use of water; and breathed into it life—namely air—whereby galvanism or the vital process arose" (*From the Greeks to Darwin*, pp. 123-7).

His work is a mass of speculation, based on Schelling's deductive method, in which new, misunderstood ideas such as "galvanism" are mingled with ancient theories such as those of Anaximander, and, especially, with abiogenesis. So I cannot agree with Driesch that "even Oken's curious theories are based on the fundamental truth of Vitalism, the irreducibility of the organic form" if they are, so much the worse for vitalism But Driesch admits that much of Oken's vitalism is impossible (*ibid*, p 98)

J. Ch Reil (1759-1813) is described as "a clear-headed biologist trained in the philosophy of his age" by Driesch, who considers his views on vitalism are best expressed in a letter to Autenreith (*ibid*, pp 98-100) Here Reil, while denouncing the name, declares in favour of a formative force associated with "dead forces" in animal organisms. So, though he offers no "real proof of the objective truth of his . . . Vitalism," Driesch claims Reil as "the first representative of a vitalistic theory founded on the concept of living matter," which is based on the endowment of "the matter with the idea." Yet Driesch asks earlier *why* the idea comes to the matter at all

Reil's philosophy is confused and his idea of some sort of force peculiar to animals is, to say the least of it, elementary. As Aristotle had a clear idea of organisms actuated by entelechy (*cf* I(*a*), 1), the transfer of this conception in a nebulous way to the "matter" of which they are composed is not, in my opinion, any advance in vitalistic teaching.

G R Treviranus (1776-1839) is discussed by Driesch among the nature-philosophers (*ibid*, pp. 100-106), and may therefore be considered here. His earlier views appeared in 1802 in the first volume of *Biology*, this term being also employed in its modern sense in the same year by Lamarck. Treviranus adopted Kant's theory of matter but regarded life as "something entirely extraneous" to it: so, too, is spiritual nature or *nous*. Vital force (*vis vitalis*) shares with external factors the energising of "formless life-substance," the nature of life consisting in the power of giving "relative uniformity to the absolute irregularity of external agents."

Though he avoids many of Oken's repulsive absurdities,

Treviranus is obscure in detail In his second volume spontaneous generation is used as a support for the view that vital force and matter are "determined reciprocally the one by the other" In the sixth, somnambulism and unusual mental states are employed to show that "something unconscious is the primary cause of life", affecting both body and spirit.

In 1831-3 (*Die Erscheinungen und Gesetze des organischen Lebens*) he modified his opinions, regarding "purposiveness for itself" as characteristic of life, which agrees with modern ideas about behaviour (*cf* III(*d*), 1). He now agreed with Stahl that "living beings and animated beings are the same." Instinct originates in an "obscure consciousness" and is "comparable to dreaming." Embryology is thought of in the same way, "as if the germ of the wheat dreamt of root, shoot and ear"; according to Driesch, Johannes Muller, Schopenhauer, and Hartmann advocated similar views later. He now regarded abiogenesis as "at least unproved"; organisation is the consequence, not the cause, of life—a sound idea (*cf.* Hunter, I(*b*), 3); and after death the elements of the body "become united by different laws than those prevailing in the former state", so bio-chemical analyses have little value This is little more than a repetition of the teaching of Stahl a century earlier, with a fanciful interpretation of instinct added.

A short account, based mainly upon Driesch's *History of Vitalism* (Ch. I (*e*), iv), may be given here of what he distinguishes as the "dogmatic school"

M. F. Autenrieth (*Ansichten uber Natur — und Seelenleben*, 1836) maintained that vital force is independent of the body, basing his thesis on the supposed spontaneous generation of lower animals, the power of fishes and of single organs to freeze and thaw back to vitality, the catastrophic theory of geology, which, he thought, showed that vital force was independent of the successive waves of organisms, and on "the facts of fertilisation", in which "the physical element is unessential" (!) Driesch considers his best argument is from instinct, about which he agreed with Treviranus. This may well be, considering that all the others are entirely fallacious and show the need for a wave of criticism to winnow

the chaff from whatever wheat there was in the fantastic vitalism of many spokesmen of this period

F. Tiedemann (*Physiologie des Menschen*, 1830) despite the synthesis of urea by Wohler in 1828 (*cf.* II(*b*), i, below), argued from the complexity of organic compounds to the existence of a vital force or forces; and from spontaneous generation to the "vital matter" of Reil Again vitalism is supported by absolutely futile and erroneous arguments. He was happier in analysing the difference between crystals and organisms

It is refreshing to turn from this rubbish to a really able writer. K F. Burdach (*Die Physiologie als Erfahrungswissenschaft*, v, 1835; vi, 1840) held that the "life principle" works through material means—"matter is only an accident, while activity is the substance of the organism" That is theory; but there is a scientific ring about the "really lucid epigenetic idea" that in development the later processes are stimulated by that which has already been formed He agreed with the iatro-physicists that the functions and mechanics of at least many parts of the organism are accounted for by the action of ordinary physical forces; but he also held that "materialism" explains details only but not their relation to the whole, which is entirely sound (*cf* III(*b*), 3, iv, and III(*d*), 2, i and v).

He is rightly critical of the vague vitalistic ideas of many of his contemporaries and predecessors; a general "vital matter" cannot really exist and would not be a sound explanation for individual lives; galvanism and heat, too, "presuppose the variety of living organic forms." But Stahl's soul is rejected, as is the "nerve principle" of J. Muller, as there is life without nerves And "vital force" only means that "there must be a peculiar cause for the peculiar phenomena of life." Burdach's own explanation for life is "the cause of existence alone"; "vital force is the primordial thought realising itself within certain limits."

Driesch considers this solution hardly satisfactory for a true scientist. But Burdach is agreeably free from the absurdities of Oken and other contemporaries, shows critical power in dealing with both mechanistic and vitalistic theories, and puts forward arguments which are still worthy

of consideration by philosophers and biologists Driesch persistently weakens the case for vitalism by ignoring the implications of biogenesis (*cf* II(*c*), III(*a*), ii, and III(*d*), 2, iii), and so does not appreciate Burdach's position sufficiently.

Burdach's metaphysical idea of Will in Nature was approved by Schopenhauer (1788-1860), who may be briefly alluded to here as a philosopher who studied human and animal existence in his own way, rejected the pre-Darwinian attempts to find an origin for man in rudimentary matter (E B, 20, 103), and held vitalistic views of biology as a science with its own laws, though modified by the opinion that organic and inorganic are both aspects of an underlying reality—feeling or will (*cf.* Driesch, *op cit*, pp 121-3).

(iii) *The Vitalistic Chemical School · Liebig, J. Muller, etc.*

The vitalism of the period is best expressed, however, not by its philosophers but by its chemists and physiologists. Their views may be illustrated by reference to three outstanding personalities—Berzelius, Liebig, and Johannes Muller.

J. J. Berzelius (Swedish: 1779-1848). His work was mostly in chemistry, but also in electricity and, in his earlier years, physiology. Like Liebig, he was created a baron

He held, with most scientists in the first quarter of the nineteenth century, that a vital force operated in living beings and could not be imitated in laboratory processes. As a chemist he was interested mainly in the chemical aspect of the problem. So he, with others, believed that, while inorganic compounds could be synthesized by chemists, this procedure was impossible for "organic" compounds, *i.e.* substances which, in nature, are found only as results of organic metabolism. In 1811 he also stated his belief that organic compounds did not usually obey the chemical laws of simple and multiple proportions ; a few years later, however, he changed his views and concluded that these fundamental—though statistical—laws governed the composition of organic as well as inorganic compounds. Berzelius himself removed one distinction between inorganic and organic chemistry in 1814 by his improved methods of analysis, which showed that organic acids possessed simple

atomic ratios like inorganic substances (Cohen, *Theoretical Organic Chemistry*, Preface). But not till 1832-1833 did he accept the doctrine of isomerism, which is of special importance in organic chemistry. Yet he first drew attention to the probability, confirmed later, that the numerous compounds in organisms might be due to the action of catalysts, analogous in their properties to platinum (*cf* III(*b*), 3, iv; III(*d*), 1, ii). Berzelius, therefore, well represents the change from acceptance of Stahlian vitalism to a much more critical position, due to the advancement of chemical knowledge to which he himself contributed so greatly

Justus von Liebig (German: 1803-1873). Liebig was one of the greatest chemists and physiologists. He was also an able teacher, and at Giessen founded the first good laboratory for the teaching of chemistry. Among other researches, he studied fulminates and cyanates, and founded the important doctrine of isomerism He was a pioneer in "pure" organic chemistry, and showed that the radicle benzoyl appeared in a long series of compounds With Wohler he published *The Radical of Benzoic Acid* in 1832, which, as Cohen (*op cit*), says, "shed a new light on the vast and unexplored region of organic Nature"

From 1838 to 1873 his attention was turned to the physiology of organisms He showed that the body heat of animals was solely derived from the oxidation and combustion of the tissues, and advanced knowledge of the feeding processes of plants, proving that their carbon is derived from atmospheric carbon dioxide and their mineral substances from the soil (Sachs, Book III, Ch. II). He was, however, wrong in attributing their nitrogen supply to atmospheric ammonia.

His *Letters on Chemistry* (1844, 1859) and *Animal Chemistry* (Eng. trans, 1843) show that he supported vitalism, like other chemists of the period He admitted that chemists could synthesise many organic substances and would produce more in the future; but pointed out that they would never be able to create "an eye, a hair, or a leaf." For such intricate systems of many different tissues, whose functions are so wonderfully correlated, the organising power or vital force of an organism is necessary. "Only an insufficient acquaintance with the forces of inorganic nature can account for the

frequent denial of the existence of a special force in organic beings, and for the ascription to inorganic forces of modes of action which are opposed to their nature and which contradict their laws" (Driesch, *History of Vitalism*, p. 119).

In *Animal Chemistry* Liebig expressly states that in animal ova, plant seeds, and plant organs "we recognise . . the *vital force*, *vis vitae*, or *vitality*"; and in animals "in the nervous apparatus a source of power" (*op. cit.*, pp. 1-3), though nerves do not produce but conduct vital force (*ibid*, p. 260). His description of the different degrees of autonomy of plants and animals (*ibid*, pp 3, 4) is similar to that of Aristotle; and he writes of "the higher phenomena of mental existence" much as Aristotle does of the "rational soul." Both "consciousness and intellect" are "a peculiar source of increased energy or of disturbance"; yet have nothing to do with development in man or beast (*ibid*, pp. 5-7).

The phenomena of animal life are determined by changes in food and oxygen "under the influence of the vital force" (*ibid.*, p. 9); but the ultimate causes are chemical forces (*ibid.*, pp 10, 34), and the phenomena are produced from chemical changes as is electricity from a voltaic cell But Liebig erred in thinking inorganic and organic phenomena equally "incomprehensible" (*ibid*, p 12); development of leaves or muscle fibres is much more wonderful than that of crystals. Unlike some previous vitalists, Liebig taught the conservation of energy; all animal heat is due to chemical action between oxygen and food material (*ibid.*, pp. 31-8, 215-19). In these passages his vitalism is weak or obscure. But in his third part he returns to the robust though enlightened vitalism of the beginning.

It is the vital force in animal tissue which causes growth in mass and resistance to the external agencies which tend to alter the tissue substance, and causes motion and change in the form and structure of material substances by altering the state of the chemical forces of the body (*ibid.*, p. 196). Its manifestations depend on "a certain form of the tissue", a fixed composition of its substance, and a suitable temperature, due to the heat of combustion of the tissues; and are soon stopped by deprivation of food. Only vital force causes these phenomena in animals (*ibid.*, pp. 198-9, 243-4, 254 *seq.*);

this force can be kinetic or potential (*ibid*, pp 200-203), and is similar to chemical forces and electricity (*ibid*, pp 209, 220), yet "it is a peculiar force" and chemical force is not the cause of vital phenomena (*ibid*, p 232), though vital energy is diminished by absence of heat or of solar radiation (*ibid*, pp 234-7).

These expressions of opinion by one who "long remained the dictator of the chemical view of life" (Singer, E B, 15, 205), made after the discoveries of Wohler and others (*cf.* II(*b*), 1, below) had thrown discredit on the crude forms of vitalism popular in the first quarter of the century, are very important. They also show that Singer's statement that Liebig "was convinced that all vital activity could be explained as the result of chemical or physical factors" (*History of Biology*, p 373), is a too mechanistic summary of his teaching. Liebig recognised that vitalists often went too far in their claims, but he held that their view was more correct than that of the mechanistic school (Driesch, *History of Vitalism*, p 119) He did not, however, originate any specific vitalistic doctrine as did the great physiologist who now demands attention.

Johannes Peter Muller (German 1801-1858) Verworn describes him as "one of those monumental figures that the history of every science brings forth but once" (Locy, p. 184); Singer ranks him "among the greatest biologists of all time" (*History of Biology*, p. 388) He was a great anatomist and zoologist; he made researches in pathology and histology, and later studied oceanic life and comparative anatomy. This great background of general zoological knowledge is significant. Muller is another example for those in danger of becoming victims to over-specialisation

Yet his fame rests principally upon his outstanding work in physiology. In the *Handbuch der Physiologie des Menschen*, published in 1833, he put physiology on a more exact as well as a broader basis, employing all possible means of research —experiment, microscopic and macroscopic observation, physics and chemistry, and psychology The connection of psychology with physiology was a new method in science (*cf* Locy, pp. 185-6; Singer, E.B, 15, 205). Like Liebig he

was a great and inspiring teacher, and his pupils, including Schwann, Du Bois-Reymond, and Helmholtz, did much to advance histology, physiology, and other branches of science, though they did not always maintain the vitalistic outlook of their teacher.

J. Muller is important in the history of vitalism for various reasons, but principally for the "principle of specific nerve energies", which he founded upon experiment It states that every afferent, sensory, nerve gives rise to the particular type of sensation which its distal end is adapted to register and to no other, whatever the nature of the stimulus it may receive; or, the sensation produced by a stimulus varies only with the nerves and sense organs affected Thus the optic nerve conveys sensations of light not only under the stimulus of light but even when the eye is struck, when the recipient is said to "see stars" This is of great theoretical importance for epistemology and the validity of scientific method; for it means that all our knowledge of external things is strictly conditioned by the powers of our sense organs and their nerves We have awareness not of things in themselves but of the ways in which they affect these organs So all our knowledge has a personal or vitalistic basis (*cf.* Singer, *History of Biology*, p 390) And this principle, as Singer has said, "remains one of the corner-stones of vitalistic theory" (E.B , 3, 613)

But Driesch does not allude to it in his *History of Vitalism.* Elsewhere (*Science and Philosophy of the Organism*, pp. 223-5), he admits that, apart from the specificity now being placed in regions of the brain rather than in the nerves, Muller's doctrine remains generally accepted But he is severely critical of it, stating that no single instance is "above all doubt" and that it is "perhaps quite false for the child."

Most authorities agree that J. Muller was a leading exponent of vitalism. Locy, for instance (*ibid.*, p. 193), points out that Muller helped to lay the foundations for modern vitalism, though his short note about this is lacking in clarity. Driesch, however (*History of Vitalism*, pp. 113-8) merely regards Muller as the last "dogmatic" exponent of vitalism, in whose writings no new idea "of really fundamental kind" is found, and credits him with only two

"separate (though not unimportant) points", though he does not define these clearly Throughout, Driesch speaks of Muller's treatment of vitalistic problems as "vague", "mixed", and so forth. Yet he gives qualified approval to Muller's teaching about the "life or integrating stimuli", which strengthen "the organic forces"; and to his treatment of death, when "the organic force is resolved into its general natural causes" and "seems to be regenerated" again through the vegetable world, though this is to me extremely vague and unsatisfactory. Nor can I concur with his criticism that Muller, like his predecessors, treated "vital force" in too quantitative a sense. But he rightly approves Muller's idea of the relative unimportance of brain substance, which is similar to that of Bergson (*cf.* III(*b*), 2, iii).

Driesch deprecates Muller unduly This is due to Driesch's own defence of vitalism being restricted to narrow though important grounds (*cf.* III(*a*), ii) Muller's significance for vitalism is firstly, that he abandoned abiogenesis and the "living substance" ideas connected with it; secondly, that in an age when mechanism was advancing rapidly he knew that it was inadequate and maintained a vitalistic outlook,[1] even if his expositions were sometimes "mixed" or "vague", the last an epithet which has been applied to the theory of the learned Professor Driesch himself Biological knowledge was very imperfect in Muller's time. Thirdly and principally, his doctrine of specific nerve energy is basic for epistemology and so for the important argument for vitalism based thereon (*vide* III(*b*), 1, iv, and III(*d*), 2, iv). It has also been extended by Verworn to other tissues of the body (*Irritability; cf.* II(*b*), iv, below).

The great contemporary French physiologist, F. Magendie (1783-1855), though experimenting in many directions upon the functions of the body, also believed in a vital force, the processes of which were inexplicable (Whetham, *op. cit.*, p 275; Singer, *ibid.*, p. 392). His more famous pupil, Bernard, departed somewhat from this attitude without committing himself entirely to mechanism (*vide* II(*b*), i).

[1] *Elements of Physiology*, pp. 23, 24, 305, etc.

# II(*b*) ANTI-VITALISTIC DISCOVERIES AND IDEAS

"Voilà mon système, ou plutôt la Verité si je ne me trompe fort. Elle est courte et simple"

De la Mettrie (*L'Homme Machine; la fin*).

## (i) *Chemistry and Physiology. Synthesis of "organic" compounds; Wöhler and urea; C. Bernard, etc.*

We must now consider the rise of much new knowledge in all departments of science during the middle of the nineteenth century, and of many new theories connected therewith. The theories were of variable merit and probability; about the general advance in scientific knowledge there is no question. These changes, which destroyed or modified the old vitalism—under which term I include all vitalistic theories before 1859—can be briefly considered under four headings, corresponding with the great branches of science, chemistry (i), physics (ii), geology (iii), and biology (iv), with a concluding reference to the philosophy of the period (v).

As regards chemistry, Priestley discovered oxygen in 1774, but remained a staunch adherent of Stahl's phlogiston theory till he died (*cf.* Foster, Ch. IX). The credit for destroying this theory belongs to the great chemist Lavoisier, who was put to death by the French revolutionaries in 1794. Yet even in 1842 Liebig referred to "the phlogiston system" as "the dawn of a new day . . . the victory of philosophy over the rudest empiricism" (*Animal Chemistry*, p xii). The usual view of the phlogiston theory as an effective bar to progress in chemistry is given by Johnstone (*Philosophy of Biology*, pp. 126-7); and we may, I think, agree with him that this fundamentally erroneous view of chemical change rather than Stahl's vitalism was a check on biological progress; for through Linnaeus, Bonnet, Wolff, Haller, and others in the eighteenth century and the vitalistic chemical school later (*vide* II(*a*), iii) biology advanced greatly in

many directions during a largely vitalistic period (*cf.* p. 261).

"The new chemistry", as it was sometimes termed in the nineteenth century, dates therefore from Lavoisier. But for many years chemical progress was mainly in the inorganic division, and belief in vital force certainly hindered its development in the study of carbon compounds. Certain substances then considered "organic" were occasionally obtained by synthetic methods between 1775 and 1828; oxalic acid from sugar and nitric acid by Scheele, 1776; formic acid from tartaric acid by Doberciner, 1822; alcohol, by Hennel, 1826; urea from lead cyanate and ammonium chloride by Wohler, 1828 But J B Cohen rightly points out that the substances from which these reactions started were partly organic in origin, and that the real history of organic chemistry only begins about 1830 (Preface, *Theoretical Organic Chemistry*, 1919)

Nevertheless, urea was a typical product of animal katabolism and lead cyanate had been regarded as "inorganic", like ammonium chloride, so too the resulting ammonium cyanate from the rearrangement of which urea is obtained. Berzelius and other leading chemists of the period had clung to the belief, due originally to Stahl, that "organic" substances such as urea could only be elaborated in organisms by a "vital force" and not by laboratory synthesis (*cf.* I(*b*), 1, iii, and II(*a*), iii) To this theory Wohler's achievement was undoubtedly "the first real blow", as Doctors Holmes and Drummond say (E B, 5, 363, 3, 589d). It has since been practically destroyed by the progress of chemistry and bio-chemistry; though no artificial synthesis of the complicated mixture of organic matters called protoplasm has ever been made, still less any mixture or compound possessing the fundamental characteristics of living matter (*cf* II(*c*), iii, conclusion).

While early work in plant physiology, based upon that of such pioneers as Priestley and Lavoisier, accorded well with vitalistic beliefs (*cf.* II(*a*), i; Sachs, Book III), later researches showed that many details formerly thought to be due to the action of vital force could be adequately explained on other grounds In 1822 de Saussure proved

that the production of heat in plants was connected with the absorption of oxygen, thus anticipating Liebig's analogous discoveries on animal heat. De Saussure's work was little regarded at first; but it was reaffirmed by Meyen in 1838. Dutrochet (French 1776-1847) worked out many details of plant physiology, which were published in his *Memoires*, 1837. He showed that many phenomena such as spore dispersal and root pressure could be accounted for, at least to a certain degree, on mechanical grounds by "endosmose" (Sachs, Book III, Ch. II) In 1822 he clarified current mystical ideas about movements in leaves, including the exceptional ones of Mimosa, and in 1826 he discovered diffusion in plants His work was supplemented by that of von Mohl (1827) upon movement in tendrils and climbing plants (Sachs, *ibid*, Ch III).

But the mechanistic development of physiology is most obvious in that of animals, including, of course, that body in which human powers and phenomena are exhibited

C Bernard (French 1813-1878) This great physiologist published his work in many volumes, including *Physiologie générale* (1872) His three most important researches were on the pancreatic ferments, the formation of glycogen and sugar in the liver, and the working of the vaso-motor system. His discoveries illustrated the fact that chemical changes in the organism can be explored just like inorganic reactions, so, as Drummond (*loc. cit*) points out, he contributed largely to the overthrow of belief in vital force as a special form of energy affecting metabolic changes. He also criticised this belief directly But this is only one aspect of Bernard's work, the one admired by mechanistic scientists.

In his *Phenomena of Life* Bernard recognised certain outstanding characteristics of living things which collectively demarcate them from non-living matter. Driesch (*History of Vitalism*, Ch. II) has shown that, though Bernard (*Leçons sur les phénomènes de la vie*, ii, 1878-1879) criticised Bichat and other less able vitalistic writers, his attack on an "interior vital principle" which acts independently of external conditions does not affect such writers as C. F Wolff, Blumenbach, and Liebig. Bernard objected to "materialists" as well as to "vitalists", because the living body possesses

arrangement and order and follows fixed laws and a "pre-established design" (Driesch, pp 133-4). "*Vital force,*" says Bernard sagely, "*directs the phenomena which it does not produce; physical agencies produce phenomena which they do not direct*" And so on So, as Driesch, who developed this argument, concludes, Bernard, though not clearly distinguishing static and dynamic teleology (*cf.* III(*a*), ii, below), must be classed among the supporters of "the enlightened vitalism", provided that his opposition to crude vitalistic ideas is also recognised.

L Pasteur (French: 1822-1895). Pasteur is claimed by Drummond as the third chief destroyer of the vital force theory. His first scientific work was in chemistry and physics, and no doubt this helped to give precision to his later epoch-making researches on "fermentation, putrefaction and disease", which "revealed not only the organisms that gave rise to these conditions, but also, in many cases the . . . physico-chemical changes that occurred." His work was ably continued by Koch and others (*cf.* Locy, Ch. XIII). But Pasteur showed, against Liebig, that the processes mentioned are all "vital", *i.e.* due to the activities of organisms (*cf.* Singer, *History of Biology*, p. 438); and his equally superb research on abiogenesis went far towards destroying one of the hypothetical pillars of mechanistic biology (II(*c*) below).

Karl Ludwig (German: 1816-1895) was a notable physiologist and a great teacher who inspired Pfluger and others. He "explained many . . . physiological events on a physical or chemical basis . . . and . . . is largely responsible for the mechanistic view of the nature of life that was and is prevalent among the leading exponents of the science of animal physiology" (Singer, *History of Biology*, pp. 390-2). This tendency was reinforced by the mechanistic development of Darwinism among German biologists (*cf.* sec. iv below, and III(*c*), 2, i).

Thus we find that progress in chemistry and bio-chemistry destroyed the vital force theory of Stahl as applied to chemical changes in organisms, and so weakened the general vitalistic position as it was crudely held in the early nineteenth century. But this progress did not and could not destroy vitalism as a

belief in the special character and autonomy of organisms which is based upon many arguments outside the province of chemistry (*cf* III(*d*), 2) And Bernard and Pasteur, like J. Muller and Liebig, were well aware of some of these, and did not attempt an all-embracing anti-vitalism.

(ii) *Physics Joule and the mechanical equivalent of heat. the conservation of energy. So "vital force" does not create energy.*

Historians of physical science agree that the great physicists of the seventeenth century, Newton, Hooke, and others, notably Boyle, followed by Cavendish, "inclined to the belief that heat was due to a vibratory agitation of the particles of bodies" But the experimental evidence was small; and in the materialistic eighteenth century heat was regarded as one of the "imponderable fluids", which included phlogiston, electric and magnetic principles, and so on (Cajori, *History of Physics*, pp 100, 106-8, 120-2). Joseph Black (1728-1799), who discovered latent heat, "explained the facts by supposing that the thermal fluid or caloric united with ice to form water as a quasi-chemical compound, and again with water to form steam" He also "originated the theory of specific heat" and established calorimetry as a branch of practical physics (Whetham, p 121) The collapse of the phlogiston theory (*cf.* sec. i above) left the caloric theory supreme in the first half of the nineteenth century; "as late as 1856 it received preference over the dynamic theory in the . . . *Encyclopaedia Britannica* (8th edition)" (Cajori, *ibid*, p. 122).

Nevertheless, early researchers had formed ideas and made experiments In 1738 D Bernouilli conceived a kinetic theory of gases, though it made no impression on the then current belief in a caloric fluid, material though weightless. Count Rumford (1753-1814) performed classic experiments on heat and work which were published in 1798 (*Complete Works of Count Rumford*, Boston). The heat evolved during the boring of cannon seemed inexhaustible, was roughtly proportional to the work done, and had no relation to the amount of shavings produced, as it should have been according to the caloric theory. Yet, though his approximate determination of the mechanical equivalent

of heat was "a considerable step toward the general idea of the conservation of energy" (Sedgwick and Tyler, p. 357) his hopes of seeing the caloric theory buried with phlogiston were not realised. Rumford's conclusion that heat is motion and not material was supported by T. Young and confirmed by the illustrious Sir H Davy, who in 1799, among other experiments, melted ice by friction though the temperature was below freezing-point; but the thermal fluid theory survived for another fifty years (Whetham, p. 245; Cajori, pp 200-203, Davy, *Elements of Chemical Philosophy*, p 94)

N L S Carnot (1796-1832) was the founder of thermodynamics His *Reflexions sur la puissance motrice du feu* (1824) were expressed in terms of caloric; but his later writings, unpublished for many years, show that he came to adopt the dynamic theory of heat and had grasped the principle of conservation of energy (Sedgwick and Tyler, p 351). He described the process by which heat may be partly converted into work, part being lost through dissipation, as a cycle. The importance of this conception was emphasised and extended by Lord Kelvin, then William Thomson (1848), Clausius (1850), Clerk Maxwell and Rankine To these the laws of thermodynamics are mainly due.

In 1851 Thomson published a mathematical proof of the second law of thermodynamics, and in 1852 he elaborated the great principle of the dissipation of energy, which means of course that while the total amount of energy remains the same, the amount that is usable decreases beyond recall (*cf* Cajori, pp 216-17; Sedgwick and Tyler, pp. 358-9). Willard Gibbs (1822-1908) may be briefly noted in conclusion as elaborating the thermodynamic equations which form the basis for much modern physical chemistry (Whetham, p. 256).

*The Conservation of Energy.*—The caloric theory was displaced by the kinetic only after the famous experiments and papers of Mayer, Joule, and Helmholtz between 1840 and 1850 Even then the scientific world was hostile and inappreciative, as it so often has been to scientifically unorthodox ideas. Yet they, especially Joule, established two

great and distinct, though allied, results, the equivalence between heat and work and the conception of heat as a mode of motion The latter confirmed the kinetic ideas of Newton and his contemporaries; the former was the basis for the triumph of the principle of the conservation of energy.

Robert Mayer (German. 1814-1878).—In May, 1842, his paper "On the Forces of Inorganic Nature" was published in Liebig's *Annalen.* "It attracted no attention, though containing the great principle that the energy of the world is constant." He also "upheld the possibility of the conversion of work into heat and heat into work" The neglect of his researches led to his insanity Only in 1858, though Germans still thought little of him, Tyndall vindicated his position in the history of science (Cajori, pp 218-9; *cf.* Whetham, p 246).

In 1842 Sir W R. Grove, English, lectured on *The Correlation of Physical Forces* (published 1846) In 1847 Helmholtz published *Ueber die Erhaltung der Kraft* These, with Joule's paper of the same year, are the first general accounts of the conservation of energy (Whetham, p 246, *cf* Rucker, *Fortnightly Review*, 1894, p. 652).

J. P. Joule (English. 1818-1889), failing to secure a professorship, "remained a brewer, but continued scientific research throughout life." To him is due the credit for establishing the mechanical equivalent of heat, upon which he experimented for forty years In his own words: "The grand agents of nature are, by the Creator's fiat, *indestructible*; and . . . wherever mechanical force is expended, an exact equivalent of heat is *always* obtained" (*Scientific Papers of J. P. Joule*, p. 158. London, 1884). In April, 1847, Joule gave in a lecture what A W. Rucker considered "the first full and clear exposition of the universal conservation of that principle now called energy", and in the following June he read a paper before the British Association. Thanks to W. Thomson, this paper attracted the attention of scientists, and Joule and he got together to pool their knowledge (Cajori, p. 220).

H. von Helmholtz (German: 1821-1894), like Mayer, Black, and Young, began as a physiologist and became a

physicist. He was also a mathematician and one of the best all-round scientists of his century. Independently of Joule he enunciated the conservation of energy in 1847; but later he admitted Mayer's priority (Cajori, pp. 221-3) Again German scientists were slow to appreciate the work of one of their own number, possibly because Helmholtz was only twenty-six at the time

After Joule had shown that the amount of energy in certain systems was constant, the work done being equivalent to the total heat produced, allowing for dissipation, this idea was gradually extended to cover other forms of energy, for instance, electrical and chemical energy and animal heat. And throughout the nineteenth century this "law" seemed to have no exceptions. It was a generalisation of the highest importance, comparable to that of the conservation of mass originated by Lavoisier. They became cornerstones of scientific philosophy as of practical physical science; and from the isolated systems for which they had been found true experimentally they were assumed to be true for the universe at large As Whetham says, "The principles passed from safe guides for empirical advance in knowledge into philosophical dogmas of doubtful validity" (*ibid*, p. 247; *cf* III(*c*), 1, below) But for many years no such suspicions troubled the minds of scientists These great principles were not then recognised as ultimately affairs of probability and statistics. They worked excellently in physical science, and very soon it was found that they operated in the organic world too, at least within the limits of experimental error.

Though the brilliant work of Faraday (1791-1867) in electricity and magnetism lies outside the scope of this book, we should note that he helped to show the dominance of physical laws in the organic sphere. "In no case," he wrote, "not even in those of [electric fishes] is there a pure creation or a production of power without a corresponding exhaustion of something to supply it" (Sedgwick and Tyler, p. 355)

Various scientists have verified within the limits of experimental error Helmholtz' prediction that these laws would apply to organisms, including Man. The famous experi-

ments of Atwater and Benedict (U S. Department of Agriculture, 1903) demonstrate that the normal human body obeys the conservation of energy as well as of mass with a display of evidence which cannot be gainsaid. Every precaution to eliminate error was taken and the experiments were numerous, varied, and prolonged. The subjects were healthy and intelligent young men

So in many respects an organism behaves like a physical system (but *cf.* III(*b*), 3, ii). Its energy is dependent upon energy gained from outside, from radiant energy as in plants, or chemical energy as in animals There is no vital force which can nullify the principle of energy, still less that of the conservation of mass. So at least it seemed absolutely in the nineteenth century, so it is to-day for all practical purposes. So again crude vital force doctrines were defeated and mechanism triumphed.

(iii) *Geology; uniformitarian ideas, Lyell, etc. Extension of idea of natural law from chemistry and physics to biology.*

The history of geology has been written by H A. von Zittel, H. B. Woodward, and Sir A. Geikie, there are good summaries by Rastall (E B, 10, 154), Locy (*op. cit*, Ch. XV), and others. Here it is only necessary to recall how, as geology became organised, it came, with the physical sciences, under the sway of those laws of cause and effect and conservation of mass and energy which strengthened mechanistic belief and destroyed many of the vitalistic ideas current in the early nineteenth century. In particular, geology clashed victoriously with the literal acceptance of the first chapters of Genesis as an exact summary of the physical history of the world, and so weakened the argument for vitalism which was derived from the belief that the Creator had miraculously made all species of organisms in a special manner, and prepared the way for acceptance of the idea of organic evolution (*cf.* II(*a*), i, and sec. iv below).

Till the last quarter of the eighteenth century geology did not exist as a science (*cf.* Woodward, p. 24); this applied particularly to palaeontology. The idea of Theophrastus (*c.* 300 B.C) that fossils were caused by some "plastic virtue latent in the earth" was repeated much later, though some

Greek thinkers saw that they were organic remains But even up to A D 1800 or so, fossils were variously held to be freaks of nature, products of inorganic changes in the earth such as "fermentations", or strange special creations, possibly deliberately placed by the Creator to bamboozle geologists and test the faith of mankind (Whetham, *op. cit.*, p. 334; *cf* Locy, Ch XV). Rationalistic persons explained sea-shells found on mountains as the discarded cockles of pilgrims, the faithful thought that the bones of great monsters indicated gigantic size for the patriarchs; and so on.

1790-1820 is Zittel's "Heroic Age of Geology." Werner (1749-1817), despite his "Neptunist" errors, taught much about early formations and something concerning the relation between fossils and the age of deposits J Hutton (1726-1797), who corrected the mistakes of Werner concerning the origin of igneous rocks, put geology on a scientific basis in his *Theory of the Earth*, and his teaching was popularised by Playfair (1802). William Smith, the "father of English geology" (1769-1839), taught the recognition of strata by their characteristic fossils and produced the famous geological map of Britain In 1807 the Geological Society of London was founded Its work, with the individual researches of Hutton, Smith, Lyell, Sedgwick and Murchison, shows how large a part English scientists took in making geology a science and in laying down the uniformitarian ideas specially emphasised by Hutton and Lyell. Incidentally we may remark that a number of the earliest members of the Society were clergymen; free thought concerning geology was by no means entirely opposed by the Church of England, though Dean Buckland did try to check the spread of anti-Mosaic ideas.

von Buch (1774-1853) was described by A. Geikie as the most illustrious geologist Germany has produced. In France Cuvier for vertebrates and Lamarck for invertebrates laid the foundations of palaeontology.

During the years 1830-1833 Sir Charles Lyell brought out his monumental *Principles of Geology*, embodying the work of his immediate predecessors, adding important observations of his own, and welding the whole into an argument for the unrelieved reign of cause and effect in the history

of the world from the earliest times down to his own, which was not only good reasoning in itself but blended admirably with the similar movements of thought which were occurring in other branches of science Though Lyell was only converted to organic evolution by Darwin's *Origin* after 1859, and though he expressly disclaimed a materialistic tendency in his work (*Antiquity of Man*, final section), it was generally held to support the mechanistic trend in philosophy and so weakened belief in vitalism.

For the principles of the conservation of mass and energy did not represent the whole mechanistic influence exerted by physical science upon biology during this period. Dalton's work on "atoms," or, rather, molecules, the kinetic theory of gases, the development of electricity, spectrum analysis and the consequent discovery of the uniformity of the elemental constituents of the universe, and other discoveries and generalisations (*cf* Sedgwick and Tyler, Ch. XVI; Whetham, Ch. V) all combined to produce an almost overwhelming sense of uniformity in nature, conveniently summarised in the expression "natural law" (*cf* Lodge, *Modern Scientific Ideas*, pp 9, 10) As Verworn (*Irritability*, pp. 21-30) and many others have pointed out, the term "law" is open to objection, and for any given effect a single "cause" is often assigned quite arbitrarily; other thinkers, such as Professor Wolf (*Textbook of Logic*, p 284 *seq*), consider that recognition of a causal relationship between antecedents and consequents is at least sometimes permissible. But, whatever phraseology may be thought least inappropriate, the fact remains that the idea of uniform becoming or absolute correlation between an event and its antecedents, however much it was derived from "medievalism" or seventeenth-century materialism (Whitehead, *Science and the Modern World*, pp. 15, 190), was found, apparently, to apply to all physical phenomena in the nineteenth century, including those studied by geologists; and so the inference was extremely strong that the relatively small organic world was also subject to this reign of uniformity. This was actually found true for many details of organic phenomena, and it seemed likely that the others would prove amenable too when physico-chemical methods were further adapted

to the peculiar conditions of plants, animals, and Man. And in 1859 one great obstacle to this view was greatly weakened, even, in the opinion of many biologists of the period, absolutely removed

(iv) *Theory of organic evolution, its bearing on vitalism by destruction, or partial destruction, of the special creation hypothesis; "Man's Place in Nature" Post-Darwinian mechanistic conceptions of Haeckel, Verworn, Loeb, etc*

The enormous importance of Darwin's *Origin of Species* (1859) in the history of biology is well shown in the writings of T H Huxley; for example·

"We wanted not to pin our faith to that or any other speculation, but to get hold of clear and definite conceptions, which could be brought face to face with facts and have their validity tested The *Origin* provided us with the working hypothesis we sought Moreover, it did the immense service of freeing us for ever from the dilemma—Refuse to accept the Creation hypothesis, and what have you to propose that can be accepted by any cautious reasoner? In 1857 I had no answer ready, and I do not think that anyone else had A year later we reproached ourselves with dullness for being perplexed with such an enquiry My reflexion when I first made myself master of the central idea of the *Origin* was,'How extremely stupid not to have thought of that!'" (Quoted by Whetham, *op. cit*, p. 299)

Huxley makes abundantly clear that Lamarck, E. Darwin and other pre-Darwinian writers had entirely failed to convince their contemporaries of the validity of an evolutionary alternative to the Special Creation hypothesis; but that this was achieved by Darwin's *Origin*. Haeckel's testimony is the same; and it is the more striking in that he was a great believer in Lamarck's central doctrine—the inheritance of acquired characters (*cf.* p. 52).

Lyell's tribute to Darwin is well known. As Osborn says· "Lyell, who believed in Natural Causation as part of his doctrine of Uniformity, had been teaching that, 'as often as certain forms of animals and plants disappeared, for reasons quite unintelligible to us, others took their place

by virtue of a causation which was quite beyond our comprehension' He had carefully studied, and rejected, the Lamarckian explanation" (*From the Greeks to Darwin*, p 227) And Professor W. W Watts made the same point in his presidential address to the British Association in 1935: "Lyell had been unable to trace any continuous development in the organic world parallel with the uniform geological development which he had enunciated, and his conversion to Darwin's view was one of the greatest tributes from a master to a pupil" (*Discovery*, Oct. 1935, p 290)

Agassiz, Owen, and some other leading biologists of the period remained unconvinced, and Kolliker and Nageli in Germany, the former more clearly and skilfully, criticised natural selection while accepting organic evolution (*cf* Singer, *History of Biology*, pp. 304-5); but they were but few, and the rising generation of scientists became Darwinians almost to a man (but *cf.* II(*d*) below). The difficulties seen by critics from 1890 onwards concerning the theory of natural selection troubled very few minds in the latter half of the nineteenth century. The triumph of organic evolution through the agency of natural selection became one of the strongest factors in producing the mechanistic outlook typical of the period In other words, "Darwinism" for most people implied a good deal more than Wallace, its co-discoverer, admitted after 1864 (*My Life*, ii, p. 17; *cf.* II(*d*), ii) or than Darwin himself suggested in his principal work.

The bearing of all this upon the vitalist-mechanist controversy requires some explanation, as it is sometimes claimed that Darwin should not be brought into it at all, that he never identified himself with materialism or vitalism, though the "tendencies" of which he made such use have a vitalistic sound. The last two propositions may be fully admitted. But on at least two points his great influence was on the side of deterministic philosophy in general and against vitalism in particular.

The special creation theory supported the dualistic vitalistic idea that the organic world is of a separate order from the inorganic and gave a background, whether acknowledged or not, for belief in organisms possessing special powers

and properties not pertaining to non-living matter; and the decay in the secular belief in abiogenesis strengthened this dualism (*cf* II(*a*), i and II(*c*) Darwin's work reduced this background considerably, presenting the whole of the animal and vegetable kingdoms as the offspring of "a few forms" or even "one" (*Origin*, last sentence) which became enormously modified under mechanistic influences. Haeckel (see below) made a speculative jump from the vantage ground provided by Darwin and assured the world that the few forms or one *must* have arisen from inorganic matter through ordinary physical and chemical changes in a dim past and so secured the foundation for his mechanistic monism

Secondly, it was not Haeckel's wild speculations but Darwin's massive learning and sombre argumentation that had most effect in shaking—for many people, destroying—another citadel of vitalism Human consciousness, apparent free-will, and most certain powers of intellect, moral and aesthetic judgment—not to speak of alleged capabilities for entering into communion with an allegedly real spiritual world—had, as they always do and will, presented a tremendous obstacle to a purely mechanical view of life Whatever might be the truth about *amoebae*, liver flukes, crabs, even vertebrates, it seemed that the mind and spirit of Man might well belong to a different order.

But Darwin opened the eyes of Huxley; and Huxley published his *Evidence as to Man's Place in Nature* in 1863, to show that anatomically Man is not more different from the great apes than their species are from one another; and that very probably Man was evolved from animal ancestry without any great break in the process (*Huxley's Essays*; Everyman's Library, pp. 96-102); concluding with the opinion that "the attempt to draw a psychical distinction is equally futile." Darwin published his *Descent of Man* in 1871 and *The Expression of the Emotions* in 1872. For the first-mentioned thesis he invoked the aid of sexual selection to a degree which many modern critics think excessive, and which A. R Wallace combated vigorously (*My Life*, ii, 18; *Tropical Nature*, Ch V; *Darwinism*, Ch X). As regards the second we may agree with Whetham that it marked an epoch and

began modern comparative psychology, "which has thrown so much light on the human mind" (*History of Science*, p 327).

Streeter, the theologian (*Reality*, p 6) Driesch, the biologist (*cf.* III(*a*), ii), and a host of minor writers agree that under Darwin's theory, "All can be explained in terms of mechanism, automatic and unconscious" And though I do not go so far as this myself (*cf* sec. v), the importance of "Darwinism" as expounded by Haeckel and others, and even as largely explicit in Darwin's own books, as an anti-vitalistic influence has, I think, been demonstrated

Psychology became largely dominated by psycho-physical parallelism, which lent itself—like Cartesian parallelism—to mechanistic interpretation of the behaviour of Man, while anti-vitalistic epiphenomenalism flourished (*cf* Driesch, *op. cit.*, p. 147; and III(*b*), 2, 1, below)

Interest in the "physiology of morphogenesis" died down though Darwin's ideas roused intense interest in comparative morphology. The emphasis now lay on what developed, not on how or why it developed. And, as Driesch remarks, this change too undoubtedly weakened the cause of vitalism (*op cit.*, p. 147).

The anti-vitalistic influence of developments in physiology has been described in section 1 above. The "cell theory," founded by Schleiden and Schwann in 1838 and elaborated by many others (*cf* Locy, Ch. XI), was another division of biological progress which came to exercise a mechanistic influence by encouraging the too exclusive study of cells against that of the organisms as a whole, as Durken points out (*Experimental Analysis of Development*, p 33). Driesch also considers the development of histology as one of the matters in which greatly increased knowledge led to the belief that all else could be comprehended in a similar way, and so encouraged the growth of scientific naturalism (*History of Vitalism*, Ch. III; *cf.* section v below).

E. Haeckel (German: 1834-1919).—The mechanistic movement in biology reached its high-water mark—or its nadir, according to the point of view—in Ernst Haeckel. From 1862 till his death he worked as a professor at Jena University, and became famous as a prolific writer on biology and atheistic monistic cosmogony.

Before passing to a brief critique of his anti-vitalistic views I would pay tribute to his great services to the cause of biology in two respects His "extraordinary power and industry" applied to the zoology of the Protozoa, Sponges, and other lowly animal groups produced a series of magnificent monographs, many of which dealt with the new material collected during the voyage of H M S *Challenger* (1872-1876). And, though he quickly added to it a mass of erroneous doctrines of his own, he was the first great German biologist to accept the Darwinian basis for organic evolution, and so to spread evolutionary ideas successfully among that scientific nation.

His *General Morphology*, 1866, was the first modern system of zoology based on an evolutionary classification. But it was marred by the introduction of hypothetical ancestral forms—Archi-Mollusca, Ideal Craniates and the like—together with other speculations which were, and are, "mere figments of imagination" (Parker and Haswell, vol. ii, p. 686, 1910 edn ). His best-known work, *The Riddle of the Universe*, appeared in 1899, being translated into English the following year Here he applied his monistic materialism to philosophy and against religion, denying vigorously the existence of God, human free-will, and immortality, primarily because of his "law of substance ", which is an illegitimate extension of the principle of conservation of energy. In *The Wonders of Life* (1904, Eng trans , 1906) he replied to some criticisms of the *Riddle* and repeated much of it with some additions.

Even W. P. D Wightman, who agrees with Haeckel's general monistic attitude to a considerable extent, admits that "Despite his optimistic statement that many of these riddles (*Riddle of the Universe*, p. 153) were in principle solved, few men of science would now make any such claim." Wightman also criticised many of Haeckel's philosophic views and his abusive treatment of those who disagree with him, concluding shrewdly that he is too addicted "to the philosophy of 'nothing but' ", from which he escapes only by the ambiguity of his terms (*Science and Monism*, 1934, pp. 257, 258).

But we are not concerned here with his philosophy. As the *Encyclopaedia Britannica* (11, 68) says, he "occupies no

serious position in the history of philosophy"; though "he is very typical of the school of extreme evolutionist thought."

Haeckel's ideas about abiogenesis are dealt with below (II(*c*), iii). He rightly regarded it as essential for a thoroughly mechanistic view of organic nature and its monistic unity with "the mechanical and natural character of the development of inorganic nature . . . established mathematically . . . by the great atheist Laplace (1799)" (*Wonders of Life*, p 376). Darwin's *Origin*, he claimed, made possible a mechanical explanation of organic nature, which Haeckel himself supplied in his *General Morphology*. He saw that the "Struggle for Life" had, or could be given, a purely mechanical significance, and ignored Darwin's wise reticence about such allied matters as the creation of life and the mystery of the variations without which natural (or any other) selection is helpless. Haeckel's so-called "fundamental biogenetic law"—that ontogeny recapitulates phylogeny—was formulated and used by him in the same dogmatic style as all his other beliefs and disbeliefs Haeckel, of course, did not discover this alleged "law" In a truer and more moderate form this generalisation was suggested by the great embryologist von Baer, and even by Agassiz, being also used by Fritz Muller and F M Balfour It is now recognised that the principle has been overworked (*cf.* Locy, pp. 230-32); and if Berg (*Nomogenesis*; *cf* III(*b*), 3, iii) proves to be right—as I consider he will, partly at least—and the Darwinians wrong, this "law" is often misleading, even in its mildest expression. To return to Haeckel—as he made psychology merely a sub-division of physiology, and the mind of man and consciousness the results of "nothing but" matter and energy (*e.g. The Riddle*, Ch. X; *Wonders of Life*, p. 345 *et seq.*) he found it necessary to provide every cell with psychic properties of a sort, and even atoms with "feelings" and "inclination" (*Riddle*, pp. 64, 78; *Wonders of Life*, p. 307 *et seq.*), all substances being endowed with "soul", which is, however, the same as energy and "unconscious."

There is, of course, no room for any vital force anywhere (*e g. Wonders of Life*, Ch. XVI), and opposition views are treated with contempt; for instance, Driesch, described apparently as a botanist (!) who accuses all "Darwinists"

of softening of the brain, and so forth, is said to have arrogance about equal to his obscurity; and so on (*ibid.*, p. 379).

Admirable though easy criticism of some of Haeckel's misstatements is given by A R Wallace in *The World of Life* (pp 3-8, 333, 363). A profusion of strange and often conflicting terminology, dogmatic statements, many now seen to be partly or wholly wrong, and abuse of all opposition characterise the mechanism of E. Haeckel.

Max Verworn (German) —Among many physiologists who have advanced knowledge by using physico-chemical methods Verworn deserves brief mention. His "biogen" hypothesis (*Physiologie*, 5th edn , pp 578 *et seq* ), "a hypothetical combination of living substance which continually disintegrates and builds up again of its own accord" (*Irritability*, p 112), is one of the many mechanistic suppositions of the post-Darwin era which have proved to be entirely baseless and untrue.

*Irritability*, which contains his mature thought on the subject, is largely concerned with metabolism. "Self-regulation" by the living substance after fatigue or disease is emphasised, and though this sounds vitalistic, he explains it as due to mass equilibrium between the quantities of food-stuffs and the quantity of "biogen" (*ibid.*, p. 112). This, he thinks, is homologous with chemical equilibrium in the non-living world, though his treatment of the one-way reaction in an organism as compared with the reversibility of analogous chemical reactions is purely speculative (*ibid.*, p. 114) There are many other such assumptions in the book, as in others of the same nature, though on experimental details he is careful and exact

His attempts at making a mechanistic philosophy of physiology and so of biology in his *General Physiology* are shown to be hopelessly confused by Woodger (*Biological Principles*, 1929, pp. 98-101); and this applies also to his efforts to criticise vitalism (*ibid.*, pp. 238-40). He falls into solipsism when he claims that physical laws are merely laws of "our own" mentality, and into *a priori* dogmatism when he postulates that vital phenomena must be governed by the "laws" that govern the material world. In other words, his

attitude is largely that of Haeckel and J. Needham (*cf.* III(*b*), 3, ii)

Another notable exponent of mechanistic ideas was Jacques Loeb (1859-1924). He too was German by birth and training, but after 1892 he worked in the United States. His chief biological work was done in physiology and artificial parthenogenesis

In *The Dynamics of Living Matter*, published in 1906, he considers "living organisms as chemical machines", claiming that "no variables are found in the chemical dynamics of living matter which cannot be found also in the chemistry of inanimate matter" (p 1) His view that "abiogenesis is the goal of biology" is considered in II(*c*), iii, below He quotes Mendel with approval (pp 3, 183), but refers to him not as a biologically minded monk but as a "teacher of physics", and fails to point out that Mendel's work deals only with already existing characters, not at all with their origin. As is natural in a book published in 1906 his notes on the work of de Vries (pp. 224-5) are now quite out of date, a criticism which applies to much else in it

That living things are profoundly influenced by their inorganic environment is undoubtedly true, and Loeb gives many interesting examples of the effects of solutions of various salts and other physical factors upon the development and life of marine animals, notably in his Lecture V. His account of inorganic artificial fertilisation in certain animals is interesting and important, and his Lecture IX tends to give the impression that fertilisation is equivalent merely to a chemical stimulus, apart, of course, from its effect in giving the male parent's characters to the offspring Yet, at the end (p. 223), he states that fertilisation is "a life-saving act", and that mature eggs usually die "very quickly" under conditions in which immature or fertilised eggs remain alive

But what a vast deal of sack for an obol's worth of bread, as Falstaff's Prince would say! There is really no comparison between this "life-saving act" upon an ovum and any reaction in inorganic chemistry. A vastly complicated series of meticulously correlated reactions is set up in one case, in the other, if a reaction is induced by some chemical it proceeds relatively uniformly and simply without increasing

complexity Further, the whole business of artificial fertilisation does not take us so far as the parthogenesis found naturally in many groups of insects, in which an ovum starts on its amazing career without any external stimulus at all. Yet who will pretend that parthenogenesis is an argument for mechanism?

In Lecture XI a glaring example is given of the futility of mechanism and of the *a priori* desire of materialists to seek any hypothetical refuge from unpalatable ideas Sachs is described as originating "the only scientific hypothesis of morphogenesis which we thus far possess." The reader then finds that this "only scientific hypothesis" rests upon a colossal assumption—"Sachs takes it for granted that the variety in the form of organs is determined by a corresponding variety in their chemical constitution" (p 199) This *petitio principi* is supported by a scanty selection of elementary botanical observations Then another, highly dubious, "assumption" of Duhamel, that the sap in plants flows in two currents, one ascending and stem-forming, another descending and root-forming, is brought in to fortify what Sachs "imagines" (*op cit*, p 200) The illustration that shoots and roots occur at the cut bases of leaves, "*e g* in a Begonia", does not prove the existence of the two mysterious saps at all For example, most leaves do not produce buds on their edges, but leaves of *Bryophyllum* do But these and similar facts do not prove the existence of two saps or the validity of mechanism All that they show is that plants vary greatly in properties, and that these properties are quite unlike those of inorganic substances

Tropisms and taxes form a favourite argument for Loeb (Lectures VII and VIII) Again we are faced with a number of interesting but disconnected facts—assuming that they really are facts—about certain lowly organisms, which differ greatly in their reactions to light, and so on; but these laboratory phenomena are too slight to form the basis of a general biological theory, such as "the origin of purposeful reactions by the blind forces of nature" (*op. cit.*, p 160). And in higher animals, especially under natural conditions, we find far more complicated and autonomous movements. Occasionally Loeb quotes experiments upon butterflies,

creatures to the study of which in a wild state in Malaya I have myself given considerable attention The highly varied and adapted reactions to light, scents, and situations generally of the very various kinds of butterflies are actually quite different from the simple taxes that Loeb describes. His theory is certainly inadequate for the numerous cases when, say, the female of a certain species will seek shade for rest or for protection, suitable parts of plants for ovi-position, flowers or perhaps dung for food, and bright sunlight for other reasons all within an hour or two (contrast *op cit*, pp 138-40)

But enough has been said to illustrate both the dogmatic *a priori* nature of Loeb's mechanism and the fallacies to which it leads. Yet this style of reasoning was common in the later part of the nineteenth century and still continues in some quarters to the present day (*cf* III(*b*), 3, ii) A brief but excellent criticism of Loeb's theories, especially of his dictum that "the problem of hybridisation and heredity *must* [italics mine—L. R W.] be transferred from the morphological to the chemical or physico-chemical school", was given by Sir W. Tilden in *Chemical Discovery and Invention in the Twentieth Century* (pp. 465-7; *cf* II(*c*), iii) How can chemistry and physics explain heredity? No amount of culture or of neglect will induce wheat seeds to produce anything but wheat So Tilden says, and if it be objected that it is internal not external chemico-physics that does the trick this is only a chemico-physical way of saying, as vitalism says more plainly, that organisms do what inorganic matter cannot. So too with propagation in animals and the inheritance of all their physical and mental properties; "in the whole range of physical and chemical phenomena there is no ground for even a suggestion of an explanation."

Other valid criticisms of Loeb's position are given by E. S. Russell in *The Behaviour of Animals* (pp. 9-10, Ch. IX, etc.), which is discussed in III(*d*), 1, ii below, and even by E. B. Wilson the cytologist (Woodger, *Biological Theories*, p. 256 *seq*); and by Durken (III(*b*), 3, v) and Johnstone (III(*d*), 1, i).

(v) *Mechanistic Philosophy*

The triumphs achieved in chemistry, physics, and geology and the establishment of belief in organic evolution by

Darwin and Wallace with natural selection as the chief means thereof produced a mass of philosophic and pseudo-philosophic works extolling materialism in general and mechanism in biology in particular. Many of these may be reasonably left to students of philosophy Some, such as those of Haeckel and Verworn, have been considered elsewhere in this book (*vide* sec. iv above, and III(*b*), 3, ii). Others demand brief mention at the close of the present chapter as illustrative of "anti-vitalistic ideas" of the period

As Whetham says (*History of Science*, p 320), the French philosophic materialism of the eighteenth century (*cf.* I(*b*), 2, iii) was revived in the nineteenth century in Germany. "Helmholtz and other physicists regarded the reduction of a problem to mass and force as an adequate solution . . But those who were not familiar with physics misunderstood them, and thought they regarded a mathematical solution as an ultimate explanation" Moleschott, Buchner (*Kraft und Stoff*), and Vogt used the science of their time, especially physiology and psychology, to produce a "materialistic metaphysic" (Driesch) in which force and matter appeared as the ultimate realities and thought was merely a secretion of the brain. Such writers are not taken as serious contributors to philosophy nowadays But in their time they and similar writers produced in the minds of many educated people, including many students of science, an atmosphere utterly hostile to any vitalistic approach to the problems of biology Only gradually was this atmosphere changed (*cf* III(*b*), 1, i, 2, iii)

Driesch deals briefly with "The Critics; and the Materialistic Reaction" of the mid-nineteenth century in his *History of Vitalism* (Ch. II). As his own arguments for vitalism are based on morphogenesis and human action (*ibid*, Part II(*f*); *cf.* my III(*a*), ii, below), he is not concerned with its apparent collapse caused by the chemical discoveries of Wöhler and his colleagues He considers that the only valid criticisms of "the older Vitalism" came from Lotze (1817-81) and C Bernard; but that both were forced to admit the existence of a considerable substratum of truth in the vitalistic position, so that their criticisms were not refutations, though they were wrongly interpreted as such at the time. This view of

Bernard is justified (*cf.* sec. i). Lotze (Wagner's *Dictionary of Physiology*, 1842; arts · "Life and Life-force, Instinct, Soul and Soul-life") was, like Bernard, justified in attacking current presentations of vitalism, but neglected those of C. F Wolff and Blumenbach; and the absence of regulation in some life processes is not, as Lotze claimed, a disproof of vitalism while its occurrence, even in some processes only, is a proof. Finally, Driesch analyses Lotze's view of the "soul" as "something entirely new in relation to the rest of nature", and finds that Lotze, though rejecting vitalism in general, held vitalistic beliefs in this limited field (*cf* E.B., 14, 406).

Like the present writer, Driesch considers that a history of vitalism cannot omit reference to "Darwinism", with which, as expounded by Haeckel, T. H. Huxley, and many other nineteenth-century Darwinians, a materialistic outlook was closely allied. He mentions four factors which profoundly influenced scientific thought in the second half of the century, as follows:

(1) The rise of a materialist metaphysic, as expressed by Moleschott, Vogt, and Buchner;
(2) "Darwinism";
(3) Robert Mayer's enunciation of the law of conservation of energy (a typically German ascription—L R W);
(4) The development of histology with the help of improved optical instruments.

Of these (2), (3) and (4) are discussed in my sections ii and iv above, (1) in this one, and Driesch's criticism of Darwinism in III(*a*), ii

For Driesch the attitude of scientists in the generation before his own was one of "absolute negation", and the "chance-theory", as he terms Darwinism, had no place for any but a mechanical interpretation of life. But here Driesch is too sweeping. Wallace combined "Darwinism" with a spiritual view of life; so did Charles Kingsley, who is quoted in the *Origin*, the late Archdeacon J. M. Wilson, and many other clergy and scientists (II(*d*), ii and iv). And while Driesch selects E. du Bois-Raymond (1848) and Helmholtz (*Vortrage und Reden*, 1884) as two of the greatest thinkers of

their age who both declare against vitalism in any form, Whetham mentions the former and his brother Paul as critics of the "dogmas of the German materialists and mechanists" (*op. cit.*, p. 342). But here Driesch is undoubtedly correct (*cf.* du Bois-Raymond, *Reden*, i, 211 *seq.*; quoted Merz, *European Thought*, ii, p. 216; and Reden, ii, 219 *seq.*, Merz, i, pp. 217-18).

Nordenskiold gives discriminating accounts of Haeckel (*History of Biology*, Ch. XIV), Buchner, Moleschott, and Vogt (*ibid.*, pp. 449-52, 521-2); and his Chapter VI is good on the development of physiology.

# II(*c*).—THE ABIOGENESIS CONTROVERSY AND ITS VITALISTIC CONCLUSION

"Life's advent was . . the most improbable and the most significant event in the history of the Universe"

SIR F GOWLAND HOPKINS (*Advancement of Science*, 1933, p 2).

## (i) *Historical.*

FOR much of this section I am indebted to the admirable summary of the history of the abiogenesis controversy given by Locy (*op. cit*, Ch XIII), though I prefer to make four divisions as compared with his three. A useful account is also given by Singer (*History of Biology*, Ch. XII).

The Bible gives little or no encouragement to the idea of spontaneous generation; living things of all kinds are sharply distinguished from non-living matter, and their creation and sustenance attributed to the Almighty. But, in spite of this, from the time of Aristotle (384-322 B.C.) down to the experiments of Redi the Italian in 1668 belief in abiogenesis was universal Aristotle taught it as a definite fact. His *Historia Animalium* is full of references to spontaneous generation for many animal groups, such as "insects", including "worms" (*e g.* V, 19-21, 551a-553a, 31, 556b; 32, 557), fishes (VI, 15, 569a), and eels (VI, 16, 570a). He also teaches it in the *De generatione* for "testacea" (III, 11, etc.), for other sessile animals, and for all invertebrates whose young are different to the adult forms (I, 1, etc.). Even in the seventeenth century Sir Thomas Browne's scepticism about the generation of mice from decaying bodies was severely criticised by one Alexander Ross: "So we may doubt whether in cheese and timber worms are generated, or if beetles and wasps in cow-dung, or if butterflies, locusts, shell-fish, snails, eels, and such life be procreated of putrified matter. . . To question this is to question reason, sense and experience. If he doubts this, let him go to Egypt, and there he will find the fields swarming with mice begot of the mud of Nilus." . . . Van Helmont (1577-1644) actually described

the way to generate mice from non-living matter (*Cambridge Readings in Science*, p. 217)

But no one really attempted to prove or disprove the truth of spontaneous generation till Redi He placed meat in wide-mouthed jars; some of these were left uncovered, others were covered. Flies laid eggs and produced maggots in the uncovered meat; in the covered vessels the meat putrefied but no maggots were produced, though the flies laid eggs on the veils covering some of them and produced maggots there. So Redi disproved abiogenesis for such creatures as maggots, and showed that they owed their existence to previously existing flies This sound experimental work was developed by Redi himself, Swammerdam (1637-1681), and Vallisneri (1661-1730), and for some time the doctrine of biogenesis reigned supreme.

But the second period of doubt arose soon afterwards. The microscopical discoveries of Leeuwenhoek and others revealed the existence of protozoa and other minute living creatures; so it was conjectured and even maintained that spontaneous generation alone would explain their origin, especially as they abound in those warm, dirty messes which have always been its accredited locations. More than seventy years elapsed before this development of the question was handled experimentally Needham, an English Jesuit priest living in Brussels, did careful experiments and published his results in 1748 He had boiled meat to get its juice, corked and sealed this is various phials, heated them repeatedly, and set them aside to cool. The frequent heatings were thought sufficient to kill all forms of life; yet microscopic organisms appeared in the solution and abiogenesis seemed to be re-established

The learned Spallanzani, also a Roman Catholic cleric, thought, correctly, that Needham's experiments were not conducted with sufficient rigour. He was the first to use glass vessels for these researches, their advantage being that they can be made with long, narrow necks which can be hermetically sealed by fusing the glass. He repeated Needham's experiments, but more carefully After his flasks had had their necks fused he immersed them in boiling water for three quarters of an hour. The organic fluids within

them remained unchanged, and no evidence for spontaneous generation was found

Needham objected that this was not a proof, as such prolonged boiling would destroy not only "germs" but the "vegetative force", *i e.* the ability to affect abiogenesis, of the solutions Spallanzani countered this by showing that even strongly boiled solutions produced numerous infusoria if they were exposed to infection from the air So biogenesis was re-established in 1775, even for micro-organisms. But Spallanzani was lucky as well as skilful; his results might have been vitiated had certain "germs" been present which can endure a temperature of 100° C. for several hours (Wyman; *vide* Locy, p 284)

It seems desirable to consider the controversy based upon Priestley's discovery of oxygen in 1774 as a separate, third, phase. It was soon believed that oxygen was essential for the maintenance of all forms of life. So the abiogeneticists now suggested that Spallanzani had interfered not with a mysterious "vegetative force" but with an essential and thoroughly well established constituent of the air in his flasks. This objection was dealt with by Schwann, the founder of the cell theory, and Franz Schultze in 1836-1837. A flask containing organic liquid was fitted with a cork pierced by two bent tubes. Through one air was sucked out by an aspirator; new air entered the other, but it was purified by passing through bulbs containing sulphuric acid and other reagents; so that unheated air, thought to be free from germs, passed into the flask The result was negative, and so again abiogenesis was disproved Tyndall, however, has pointed out that again the biogeneticists were fortunate, as their precautions were not perfect (*op cit.*, p. 285). They were sufficient for the period, however, it was clear that the generation of micro-organisms depended on something in the air, and in 1843 Helmholtz showed that this could not pass through a moist animal membrane and was therefore a solid.

The question was, however, reopened once more. Pouchet in France in 1859 and Bastian in England in 1872 decided on *a priori* grounds that abiogenesis *must* occur, and in this frame of mind set out to prove that it did occur, despite the general scientific opinion of the day, based on sup-

posedly valid experiments, that it did not. And not for the first time, or the last, in the history of science, believers in a theory were able to establish it by experiments until abler and less prejudiced observers took the matter up anew Pouchet did the old experiments and got different results which supported his desired conclusion He also went further and executed a new one, which was ingenious, though in fact not as perfect as he thought.

He filled a flask with boiling water, sealed it, opened it again under mercury, and connected it with a retort in which oxygen was generated This artificially produced oxygen, free from germs once the air in the retort had been displaced, bubbled into the flask of, presumedly, sterilised water, and forced some of it out. Pouchet heated a pinch of hay strongly, gripped it with sterilised forceps, and pushed it under the mercury and into the bottle In a few days the boiled water in a flask supplied with pure oxygen was teeming with life The proof of abiogenesis seemed perfect

To settle this important matter a committee was appointed by the French Academy of Sciences, and it invited the aid of Pasteur. In 1864 he exposed the flaw in Pouchet's experiment Using a beam of light in a dark room, he showed that the surface of the mercury was covered with dust and that particles adhered when hay or similar bodies were pushed through it He followed this up by numerous experiments showing that living germs actually float in the air, and that some can live without free oxygen He also demonstrated the efficacy of a plug of cotton wool for protecting sterilised fluids from germs while admitting air.

Translated extracts from Pasteur's account of his experiments and conclusions are given in *Cambridge Readings in the Literature of Science* (pp 216-29). His brilliant researches are often quoted as the end of the biogenesis controversy.

But his results were still further established by J. Wyman of Harvard and Professor Tyndall. Wyman showed that many spores can resist boiling for several hours, so the modern method of discontinuous heating was introduced· by this spores which resisted the first boiling are killed subsequently when they have begun to germinate and their protective covering has become softened. Tyndall (1820-1893) con-

tinued Pasteur's work in reply to the claims of Bastian His particular contribution was based on his discovery of optically pure air, and a good summary of the details of this beautifully planned experiment is given in Locy (pp 290-92). The result was to demonstrate conclusively that no generation of organisms ever occurs unless the "germs" or spores of existing organisms are allowed admittance from the atmosphere

Locy sums up very guardedly by saying that, "So far as the evidence goes, the spontaneous origin of life under present conditions is unknown" (*op cit*, p 293).

Singer considers Pasteur's as the critical experiment, adding that similar experiments have been devised when necessary to rebut opposition So abiogenesis (there is an obvious misprint in the *Encyclopaedia* here) is non-existent "under present terrestrial conditions" This, he points out, gives the biologist "a conception of the nature of life comparable in its value as a standard of scientific research to the doctrine of the conservation of matter and of energy in the hands of the physicist." So he rightly includes biogenesis as one of the six causes which revolutionised the outlook on the nature of living things in the period from about 1860 to 1880 (E B., 3, 617, *cf* *History of Biology*, p. 442).

Sir P. Chalmers Mitchell states the position with commendable force and clarity. "No biological generalisation rests on a wider series of observations, or has been subjected to a more critical scrutiny than that every living organism has come into existence from a living portion or portions of a pre-existing organism" (E B, 3, 593).

This is the position adopted in this book, together with the "conception of the nature of life" described by Dr. Singer. It cannot be too strongly emphasised that theories about abiogenesis in the remote past, under different conditions, or in the future (*cf.* sec. iii below), however plausible they may appear or however attractive they are to those who wish to see biology contained within a mechanistic framework, are theories and theories only, using the word in its vaguest sense For the historian of science and the student who seeks biological knowledge firmly based upon experiment biogenesis —*omne vivum ex vivo*—is the universal rule.

It is to be noted that belief in biogenesis is supported pragmatically in countless ways, by antiseptic and aseptic surgery, by canning and other industries for the preservation of food, and so on, all these being successful practical applications of the proven hypothesis that no life arises except from pre-existent forms of life.

(ii) *Its bearing upon Vitalism.*

Aristotle seems to present us with such a clear-cut vitalistic scheme, acceptable to such modern thinkers as Driesch (*History of Vitalism*, pp 20-21), that it is startling to realise that he believed that some plants and many animals, possessing two grades of "soul," habitually arose by spontaneous generation. But this belief must be noted as a weakness in his vitalism which is too often passed over by his admirers, though it accords well with his conception of continuity in Nature (*De partibus*, iv, 5, 681a; *Historia animalium*, viii, 1, 588b, quoted by Singer, *Greek Biology*, pp 30, 31).

The establishment of biogenesis as the unbroken rule for all organisms is an achievement of the later nineteenth century. By it various vitalistic theories stand condemned, including those of Oken and other "nature-philosophers", in which the abiogenetic origin of "infusoria" and hence, indirectly, of all higher forms is fundamental (*cf* II(*a*), ii). Lamarck's "philosophy" has been held to be vitalistic in trend (*cf* Driesch, *Science and Philosophy of the Organism*, pp 176-9), but he too made his supposed chain of organisms continuous with inorganic matter through abiogenesis (*Zoological Philosophy*, pp 176, 187-9, 236-44). Some modern philosophers, few of whom are trained biologists or outdoor naturalists, approximate to the idea of abiogenesis in various ways, such as General Smuts in Holism and Alexander, Lloyd Morgan, and others in Emergent Evolution (*cf.* III(*b*), i, ii and iii).

Driesch does not deal with the origin of life in his argument for entelechy, but says that we know nothing about it (*Science and Philosophy of Organism*, p. 302). This is also the view of Sir F. Gowland Hopkins (*Advancement of Science*, 1933, p. 2).

Vitalistic theories that are based upon abiogenesis must then be rejected as wrong; and indeed they have little to commend them on other grounds. The important question then arises whether biogenesis supports mechanism or other forms of vitalism It undoubtedly supports a vitalistic view of the phenomena of life, as indicated in the preceding chapter Life, whatever else it may be, is a unique thing in the world; it is not derivable from the ordinary forms of energy, though they are interchangeable among themselves, nor can it be obtained by juggling with inorganic matter. Certain things—organisms—possess, or perhaps it is better to say are possessed by, this unique characteristic, life; it is therefore natural that they should behave differently to things that are not endowed with it, just as physical systems possessing a difference in potential behave differently from a system containing the same amount of matter and energy but without difference of potential. It is not only possible but probable that living things should exhibit not only the normal reactions of their material parts considered as chemical molecules or as electrons and protons but other actions which non-living aggregations of molecules, electrons, and so on do not perform. Biogenesis makes a vitalistic outlook on such phenomena a reasonable one.

But the existence of such reactions can and must be debated on its own merits. The mere existence of the phenomenon we call life, which is unproducible except from a pre-existent source of life, is itself a valid and distinct argument for vitalism, the doctrine that living things are autonomous and exhibit powers which inorganic matter does not possess.

(iii) *Objections to the universality of biogenesis. The origin of life.*

Dr. Singer, after admitting the validity of biogenesis for all organisms under present conditions, is careful to add that this doctrine does not prejudge the question as to the first origin of life, nor the possibility of its having arisen at different times and in different places (*History of Biology*, p. 442). This attitude has been common among biologists from the first establishment of biogenesis down to the present day; T. H. Huxley was one of its chief exponents

though he supported Pasteur (*Essays VI*· Everyman's Library; *cf.* E.B., 1, 48). This attitude therefore merits consideration here.

Primitive forms of life must have first appeared either on this planet or elsewhere The second theory was put forward by the physicist Lord Kelvin and others; it suggested that living germs were conveyed to the earth in crevices of meteorites or on or with cosmic dust (panspermia). But, as Sir J A Thomson said (E B , 3, 608), it is not an easy hypothesis, in view of the need of protoplasm for moisture and a certain limited range of temperature. There is of course not a particle of actual evidence for it, and, anyhow, it only pushes the difficulty a step further back If living matter could not arise on the earth, to the conditions of which it is closely adapted, it is very unlikely that it could (*a*) arise more easily in another star or planet and (*b*) survive transportation through the cold and vast distances of interstellar space The notion that life as it is known to-day is as old as matter is simply preposterous

"The only other scientific suggestion", according to Thomson, is in fact abiogenesis on the earth in the past, a hypothetical process sometimes termed archebiosis It is extremely probable that the earth's atmosphere contained much carbon dioxide in early times, and abundant warmth and moisture can be safely postulated. Baly and other chemists have synthesized formaldehyde and sugar, and even nitrogenous carbon compounds from formaldehyde and nitrites, using only ordinary sunlight with a "metallic photo-catalyst" But, after giving this brief summary of the chemical experiments upon which belief in archebiosis is mainly based,[1] Thomson goes on to refer to the living organism as apparently a new synthesis which exhibits "qualities that cannot be adequately described in terms of matter and energy"; so that some autonomy can be claimed for biology on his view (E B , 3, 608-9).

The definitely anti-vitalistic, monistic reply to the conclusive experiments of Pasteur and Tyndall may be illus-

[1] It seems unnecessary to refer in any detail to such theories as that of *The Origin of Living Matter*, by Gray and Bligh, 1933, which depends on "binary atoms" supposedly produced by the energy set free by the splitting off of the moon from the earth.

trated by extracts from Haeckel (*Wonders of Life*, Ch. XV, 1906) and W. P. D. Wightman (*Science and Monism*, 1934).

Haeckel represents Darwin as saying that he "'has nothing to do with the origin . . of life itself.'" This Agnostic position, he adds, is held by a number of distinguished modern (1906) scientists He has already dismissed supernatural creation in any form as "based on pure superstition", though held by the botanists Wigand and Reinke (*op. cit.*, p. 352) and actually expressed by Darwin in all editions of the *Origin* (concluding section). He also rejects, I believe rightly, the views of Kelvin, Helmholtz, and others, that life has reached this planet from meteors, and of Fechner and Preyer that life is due to an ill-defined life in the elements of the world, both suggesting an eternal existence of "life."

His own hypothesis, termed variously "autogony", "archigony", or abiogenesis, was first formulated in his *General Morphology* (1866). The various conditions necessary for life are enunciated fairly, with a reference to the early incandescent state of the earth, which all educated people accept nowadays. He then solves the difficulty blandly by stating that, when the earth's crust became cool enough, the first chemical processes to occur "must have been catalyses, which led to the formation of albuminous combinations, and eventually of plasm" (*i e* protoplasm). Hence the earliest organisms must have been "plasmodomous monera" or "structureless organisms"; then came probably homogeneous globules of plasm, and then the first cells (*op. cit.*, pp. 355-6). The likelihood of this sequence "appeals directly to the biochemical facts of vegetal physiology" ; and so forth.

Pfluger elaborated a "cyanogen-theory of abiogenesis" based on the "chemical fact" that cyanogen and its compounds, also carburetted hydrogen, are only formed at incandescent heat. "'Hence life was born from fire'" . . . and so on. This is used to supplement the "monera-theory" of Haeckel (*op. cit.*, pp. 359-62, 370).

He naturally also quotes the great botanist C. Nagali in support, to the following effect:

"'The origin of the organic from the inorganic is . . . not a question of experience and experiment, but a *fact* [italics mine—L. R. W.] deduced from the law of the constancy of

matter and force. If all things in the material world are causally related, if all phenomena proceed on natural principles, organisms, which are formed of and decay into the same matter, must have been derived originally from inorganic compounds" (*A Mechanical-physiological Theory of Evolution*, 1884).

Thus, even in 1906, were believers in materialistic and atheistic monism confirmed in the true faith and shielded from the temptation to think that there might possibly be anything unusual about the origin of life To my mind this passage is nonsense; viewed more kindly, the claim for a deduced "fact" being set aside, "this excellent and clear declaration of a distinguished scientist and profound thinker" (Haeckel, *op cit*, p. 358) simply boils down to the truism that organisms obtain their matter from inorganic matter and return it to inorganic matter when they die—and also during katabolism, Nagali might have added

A summary of the work of Redi, Spallanzani and Pasteur is given fairly; but it is explained that such work does not destroy the theory of abiogenesis but only what Haeckel terms "saprobiosis" or "the formation of lower organisms out of the putrid . . . elements of higher organisms" (*op cit*, pp. 363-7). Haeckel claims that the conditions of existence when life first appeared were "totally different" from what they are now; but, like some more modern theorists, he forgets that the more unlike the conditions were the more unlikely is it that a form of life then produced, if it ever was produced, would adapt itself to "totally different" conditions later.

Wightman gives the views of an able modern monist; for obviously abiogenesis would be an important link in securing that unity between living and non-living matter which is desirable or essential for monism as it is usually interpreted; and Wightman criticises ordinary mechanistic theories in biology as uncompromisingly as he does vitalism (*op cit.*, pp 345—"silly theories", 346, 347). He rightly rejects the "facts" and "reasons" which Haeckel and Nagali thought so convincing in the late nineteenth century; for at its close "the problem appeared to be insoluble" (*op cit.*, p 355). But, though admitting that dogmatic assertion is out of

place at present, he turns hopefully—as do many other materialists of the present day—to the filterable viruses as substances which are hard to define as living or non-living and therefore may provide the "missing link" (*op. cit*, pp. 355-7, with reference to H H Dale, *The Biological Nature of the Viruses*, *Nature*, 10th Oct 1931). He points out that their discovery is due to the diseases they cause, and thinks it is "no wild speculation to suppose that . . there are many other such bodies whose function is the creation and sustaining of life, but which . . must at present blush unseen." If so, the gap between living and non-living matter is bridged.

This is certainly plausible, but he has just pointed out that the evidence for biogenesis is not merely negative, "since nutrient media, which have remained sterile for years . . have, on the entry of a few suitable organisms, within a few days teemed with the latter's progeny" (*op. cit*, p 355). And, we must remember on the authority of an expert on this subject, "so far, a virus has always required the presence of living cells to propagate itself" (*Filterable Viruses*, Rivers, p 97)

Such knowledge as exists at present about filterable viruses is largely scattered among medical and scientific journals But the Royal Jubilee number of *Nature*, 4th May 1935, contains valuable summaries of the last twenty-five years' progress in this and other branches of science, written without philosophical bias Dr. J. A. Arkwright, F.R S., does not confine his survey of viruses to their causation of disease, despite the title of his article According to him, most pathologists believe that they are similar to "excessively small" bacteria, but this idea is partly based on analogy only, "since viruses cannot be subjected to the same tests as bacteria." Bacteria can usually be propagated "on sterilised artificial culture media", but a virus requires living cells, "in most cases" within an actual host This fact has suggested that a virus is not necessarily living but is a mere stimulus to the host cells, as in sarcoma of fowls, and even that a virus is a product of its host. The latter view, however, is opposed by the propagation of "diverse viruses in the same kind of animal" and by the same virus infecting widely different

species (*op. cit*, p. 719). But "the chief reason for doubting the living nature of some phages and viruses is the very small size of their filterable particles" Doerr grants that some viruses have been shown to be living, but denies the possibility of life in the smallest ones Boycott has suggested that the accepted definitions of life may not be universally applicable, or that "some intermediate state between what is called living and dead matter" may exist (*op cit*, p 720). But Arkwright points out that the metabolism of an organism necessarily parasitic "inside the cell of its host" is probably much simpler than that of an independent organism Finally, "inclusion bodies" and some of the symptoms due to a virus can "in special cases be imitated by the addition of certain inorganic salts to the soil, but the disease is not then transmissible"; while "in many ways the viruses of plants resemble those of animals" (*op cit*, p 721)

Much remains to be learnt about viruses and their allies, and the possibility of a new semi-living category of entities, as adumbrated by Professor Boycott, still exists If this be so, Wightman's monistic speculation concerning unity between organic and inorganic must be admitted, at least to a certain extent But it has not been established And even if it were, what would it prove about biogenesis? The old riddle about the hen and the egg may be unanswerable, but a pathogenic, parasitic thing—whatever its precise nature might be—is certainly the successor, not the predecessor, in time of the higher organisms upon the tissues of which it depends for its existence

This, I found after writing it, agrees with the opinion of Sir F Gowland Hopkins in 1933: "If they (filterable viruses) are nevertheless living, this would suggest that they have no independent power of obtaining energy and so cannot represent for us the earliest forms in which life appeared" (*Advancement of Science*, 1933, p 3).

So far then as the methods of science can be applied, the position of biogenesis as a proved fact stands as it has stood ever since vague beliefs in its opposite were confronted by exact experiment and prolonged observation

As regards the future, the materialist J. Loeb (*cf.* II(*b*), iv)

has also emphasised the theoretical importance of abiogenesis He admits that nobody has yet observed it, but considers it not impossible, and states that "experimental abiogenesis is the goal of biology" He is, however, careful to avoid premature blunders such as that of T. H Huxley with the so-called "Bathybius", and points out that it is insufficient to produce proteins synthetically or colloidal substances that show mere external resemblance to living cells, but that the artificial product must be "capable of development, growth, and reproduction" (*Dynamics of Living Matter*, p 223).

But Berg, though opposing vitalism as he understands it (*cf* III(*b*), 3, iii), powerfully criticises Loeb's conception of living beings as chemical machines as metaphorical only; "No machine properly so-called possesses any of the characters mentioned by Loeb." Regarding Loeb's hope that future science will be able to construct a "living machine" he admits that opinions may differ, "but in any case we are entitled to base such an assumption on the one premise that the 'living machine' will be constructed by another 'living machine', *i e* man . . a strictly mechanistic conception of life can be held only on the assumption that the 'living machine' be constructed by the unaided forces of inorganic nature alone But such an assumption is no more justifiable than the expectation of finding in the state of nature a watch, or a steam engine, or a volume of *War and Peace*, composed by the blind agency of atoms without any intervention on the part of the human mind. So far, at least, the principle of *omne vivum ex vivo* alone holds the ground" (*Nomogenesis*, p 2).

The biogenesis controversy was dealt with by the President of the Zoological Section of the British Association in 1933. The conclusions of Dr. J. Gray, F.R S., are practically identical with my own, though his treatment is somewhat different. It runs as follows

Schafer's belief, like Haeckel's, that living matter "owes its origin to causes similar in character to those which have been instrumental in producing all other forms of matter in the Universe" (British Association, 1911), was supported by Ray Lankester and others at the time, and, more recently,

though more cautiously, by T H. Morgan and, Dr Gray thinks, by "not a few modern zoologists." There is considerable truth in the view that "the most far-reaching mechanistic views have been and are being put forward by biologists, the more cautious . or . vitalistic views are held by physicists and chemists" (*cf* Sir W Tilden, quoted later in this chapter). But Gray quotes J. S Haldane and A V. Hill as physiologists who regard "the purely physical outlook with distrust"; and Dr Gray and his successor in 1934 are examples of modern biologists who regard this view with more than distrust (*Adv. of Sci*, 1933, p. 83; *cf* III(*b*), 3, and III(*d*) below).

Dr. Gray then analyses the abiogenetic position from the probability point of view. Though it seems plausible at first sight from the evolutionary standpoint, and though "as a physical phenomenon it is undoubtedly *possible*", abiogenesis "is so improbable that . . . in any other sphere of human thought it would be discarded as a figment of a deranged brain. Why should biology accept a standard of probabilities incomparably less satisfying than that of other branches of knowledge?" As regards statements that "in past ages, events which are now very improbable were in fact of quite frequent occurrence", he replies that, "as scientists we cannot accept this . without some assurance as to . . . the nature of the conditions which made the origin of life inevitable or even probable . . . is it not strange that every attempt to reproduce them in the laboratory has completely failed?" (*op. cit*, pp 84-5).

Putting the facts in another way, we see that in the physical world systems appear to move towards the state of greatest probability, and the structure of a dynamic system to disintegrate rather than to grow more elaborate. "Is there any evidence which suggests that, within the physical world, a dynamic machine has spontaneously come into existence? . . . has it . . . ever occurred under the observation of mankind? Unless a positive answer can be given to this question the belief in the spontaneous generation of living matter seems to be a negation of the principles which underlie scientific thought" (*op. cit*, pp. 85-6).

We may well conclude with his quotation from Niel

Bohr: "The existence of life must be considered as an elementary fact that cannot be explained, but must be taken as a starting-point in biology" (*op cit*, p 92).

The views of a great chemist on the differences between a minute organism and a non-living speck of colloidal matter were ably described in 1916 by Sir W. Tilden, F.R.S., whose argument may be summarised as follows:

(*a*) We know nothing about the fate of a very large molecule, supposing such a unit could be formed apart from living matter; but to suppose that such molecules would manifest signs of life is purely speculative

(*b*) Chemical syntheses normally involve the use of concentrated reagents and high temperatures in processes quite unlike those "which must be assumed to go on in living tissue." This remains true on the whole, despite the interesting recent syntheses of comparatively simple compounds under fairly natural conditions outlined above.

(*c*) And if the development of formaldehyde from water and carbon dioxide by sunlight and other similar syntheses did lead to the formation of living matter, why is this process never found in nature despite all the experiments devised to prove abiogenesis?

(*d*) Again, even supposing that a complex colloidal protein was elaborated abiogenetically, no chemist would believe that it would cease to exhibit the ordinary chemical characters of such substances and acquire instead the power of utilising energy from external sources and, in short, of behaving like an organism

(*e*) Apparent resemblances between cell membranes and the films and cavities formed under certain conditions by inorganic colloids are no more homologous than those between fossil plant remains on the one hand and dendritic markings or frost foliations on the other.

(*f*) Protoplasm is more than a solution of colloids and saline electrolytes It possesses viscosity and cohesion. A drop of colloid solution gradually diffuses away in water; an amoeba does not

(*g*) The thickening of the film on a colloid like glue has no limits. But the formation of a new cell membrane in a lowly organism is regulated; it proceeds to the amount

required and then stops, for the organism, however lowly, is autonomous. Cellulose molecules, for instance, are added to a plant cell wall till the required thickness and area are reached; then the process, which originates from a substance which is not cellulose and does not normally contain it in itself, ceases, to be renewed again perhaps when a new cell division occurs

(*h*) Chemistry and physics cannot explain the differentiation of new substances in organic cells nor the direction or autonomy which controls their production and is certain evidence of a vital principle—or whatever name is given to it.

(*i*) Heredity also is inexplicable.

(*j*) So, too, reproduction in animals No physical or chemical "laws" explain the reproduction, in an adult derived from microscopic sexual cells, of mental as well as physical attributes of parents and ancestors

This master of chemistry wisely concludes that "those who accept the purely materialistic doctrine as to the origin of life have before them the necessity of establishing a vast number of facts before such doctrine can be made generally acceptable to the scientific world . . . it is urgently desirable that . . . the biochemical and physiological student will not allow his enthusiasm to colour his hypotheses independently of the light which can be cast upon them" (*Chemical Discovery and Invention in the Twentieth Century*, pp 462-8)

And J. B. Cohen, F.R S , holds the same views: "Between the synthesis of the most complex organic substances and that of the simplest living cell there exists, and probably always will exist, an impassable gulf" (*Theoretical Organic Chemistry*, p. 5). Fabre puts the same idea in vivid language (*vide* II(*d*), i).

So there seems no reason to deny the explicit teaching of Darwin and Wallace that the appearance of life must be due to a definite creative act by the Supreme Being (*cf.* II(*d*), ii), which agrees with Bohr's "elementary fact", unless scientists prefer the agnostic attitude of Driesch and Hopkins The former seems to me the more logical, even if it goes outside the formal boundaries of "science." But even in the latter

case the fact of biogenesis is admitted, obviously fraught with vitalistic meaning.

Of the many vitalistic theories we have to consider none has been more thoroughly tested than that of biogenesis, and none stands more firmly proven at the present time.

# II(*d*). — VITALISTIC BELIEFS AFTER 1859, HELD DESPITE THE PREVAILING MATERIALISTIC TREND IN BIOLOGY

"I have long recognised how much clearer and deeper your insight into matters is than my own "

C. DARWIN to A R WALLACE (MARCHANT'S *Life of Wallace*, 1, 183)

## (i) *J H Fabre (French: 1823-1915) and instincts*

ALTHOUGH the mechanistic opinions described in Part II(*b*) continued to be widely held among biologists from the publication of the *Origin of Species* until well into the twentieth century, there were not a few profound students of living nature who maintained an independent outlook and expressed views of a strong vitalistic type. Two of the most important of them are J. H. Fabre and A. R. Wallace, though Fabre's work remained neglected for the greater part of his long life

Both were born in the year 1823 and lived for over ninety years; but their main work was done before 1895 Both were largely self-educated and so unusually independent in thought. But while Wallace was a great traveller and discoverer and wrote on a vast number of subjects connected with biology, Fabre spent his whole life in southern France and Corsica and, though a keen student of local botany and geology, is known only for his remarkable studies on local insect life; and his opinions on vitalism, instinct, and other general questions have to be culled from the pages of his voluminous *Souvenirs Entomologiques*.

Fabre, however, preferred to call himself a naturalist rather than an entomologist (Legros, *Life of Fabre*, p. 135); and an excellent naturalist he was, combining experiment with observation, intuition with inductions drawn from nature. His work suffered from lack of knowledge of that of contemporary investigators; but it was correspondingly original and sincere. "Fabre will suppose nothing; he will

only recognise the facts . . and for the rest . . . reply simply that 'we do not know'" (Legros, p. 190).

A few typical quotations must suffice to illustrate Fabre's views, based on no *a priori* conceptions, but on unremitting study of organisms, extending sometimes to twenty-five years for a single life-history Most of these are taken from *The Life of the Grasshopper* (trans. A T. de Mattos, 1917); but the following extract from Legros (p. 208) illustrates Fabre's opposition to evolution and his belief in innate faculties :

"Species are therefore born as a whole, each at the same time, at the same moment, bringing into being its new organism with its individual peculiarities, its indelible and innate faculties and tendencies, like so many medals, each struck with a different die, which the gnawing tooth of time attacks only sooner or later to annihilate it."

Referring to the singing insects which belong to older orders than the beetle, bee, butterfly and others, he says: "This shows, from the mere point of view of sound, the futility of those theories which try to explain the world by the automatic evolution of progress nascent in the primitive cell. All is yet dumb; and already the insect is stridulating as correctly as it does to-day Phonetics start with an apparatus which the ages will hand down to one another without changing any essential part of it. Then . . . we find something worse than marking time, we find an enormous retrogression, until one last bound brings us to man's own larynx" (*The Grasshopper*, p 257).

The last sentence illustrates one of Fabre's faults, a tendency to exaggeration when he turns to generalities from his usual work of observing and recording details of insect life; the omission of reference to the songs and cries of birds, even to the croaking and whistling of Batrachia, is unpardonable. Yet it is strange that the higher orders of insects do not possess the sound-making powers of their more primitive allies.

"Can it be that the theory of progressive acquirements is only a specious lure? Are we to abandon the savage theory of the crushing of the weak by the strong, of the less well-endowed by their more highly-gifted rivals? Is it permissible to doubt, when the evolutionists talk to

us of the survival of the fittest? Yes, indeed it is!" (*ibid.*, pp. 272-3).

The Pedestrian Locust "possesses the germs of wing and wing-case . . and it does not think of using these germs by developing them. . . . The larva is born with the hope of flying at maturity. As a pledge of that fair future, it carries on its back four sheaths in which the precious germs lie slumbering. Everything is arranged according to the rules of normal evolution Thereupon, suddenly, the organism does not fulfil its promise, . . it leaves the adult insect without sails . . . with only useless rags

"Are we to lay this nudity to the charge of the harsh conditions of Alpine life? Not at all, for the other hoppers, living on the same grassy slopes, manage very well to achieve the wings foretold by the larva's rudiments

" . . Now tell me, if you please, what reasons persuaded the Pedestrian Locust not to go beyond his rude outline of a flying-apparatus. He also, surely, must have felt the prick of necessity for ages . . and all the endeavours of his organism, striving to achieve a better lot, have not yet succeeded in spreading bladewise his incipient wings

"If we accept your theories, under the same conditions of urgent necessity, diet, climate and habits, some are successful and manage to fly, others fail and remain clumsy pedestrians. . . . I abandon the explanations offered . . . In the presence of the unfathomable problem of origins, the best thing is to bow in all humility and pass on" (*ibid*, pp. 374-7).

"The stones of our buildings are arranged in accordance with the architect's considered plan . . . Similarly, a Locust's wing . . . speaks to us of another Architect, the Author of the plans which life must follow in its labours.

" . . . We cannot see a blade of grass grow; but we can easily witness the growth of a Locust's wings and wing-cases.

"We stand astonished at this sublime phantasmagoria of a grain of hemp-seed which in a few hours becomes a superb piece of linen. What a proud artist is life, driving its shuttle to weave the wings of a Locust, one of those insignificant insects of which Pliny, long ago, said: '*In his tam parvis, fere nullis, quae vis, quae sapientia, quam inextricabilis perfectis!*'" (*ibid.*, pp. 420-23).

From contemplation of one of the organic phenomena not paralleled in non-living matter Fabre turns to the attempts made to synthesise living matter

"Very well, be it so; you have thoroughly prepared your protoplasm . . . your wishes have been fulfilled, you have extracted from your apparatus an albuminous glair, which goes bad easily and stinks like the very devil in a few days' time; in short, filth What do you propose to do with your product?

"Will you organise it? Will you give it the structure of a living edifice? Will you take a hypodermic syringe and inject it between two impalpable films to obtain were it only the wing of a gnat?

"For that is more or less what the Locust does He injects his protoplasm between the two scales of the pinion; and the material becomes a wing-case, because it finds as a guide the ideal archetype of which I spoke just now. It is controlled . . . by a plan which existed before the injection, before the material itself.

"Have you this archetype, this co-ordinator of forms, this primordial regulator, at the end of your syringe? No? Then throw away your product! No life will ever spring from that chemical ordure" (*ibid*, pp. 420-23)

"Organised matter . . . escapes the formulae of the laboratory. *Its chemist is life*" (*ibid*, pp. 568-9, italics mine—L. R. W.).

Fabre knew better than anyone else the intensity of the struggle for existence in the insect world, the countless births and deaths, and the savagery of the nuptials of the Mantes, scorpions, and other arthropod types But he knew that there was too, sometimes at least, a dissipation of energy unconnected with feeding or breeding; that insects danced and sang for pure *joie de vivre*, without a mechanical compulsion.

"What purpose is served by the Grasshopper's sound-apparatus? I will not go so far as to refuse it a part in the pairing. . . . But this is not its principal function. Before anything else, the insect uses it to express its joy in living, to sing the delights of existence with a belly well filled and a back warmed by the sun, as witness the big Decticus and

the male Grasshopper, who, after the wedding, exhausted for good and all and taking no further interest in pairing, continue to stridulate merrily as long as their strength holds out . Even so most often does the singing insect stridulate; it is celebrating life" (*ibid*, p 270) So too the Italian Locust (*ibid*, p 371)

Fabre's vitalistic ideas, based on the longest life of assiduous study ever given to the insect world, are, however, best illustrated by his views on instincts These, which insects display so lavishly, are for him not the product of ages of selection by an automatic process but something *sui generis*, something essential to living creatures, "implanted" in them from that beginning about which he disclaims knowledge, strangely variable even in closely allied forms, inexplicable as life itself and inviting the most careful observation and description In many cases, that of the *Sitaris* larva for example (Legros, p. 182; S E, 2nd series, Ch. XV), education is out of the question; the tiny grub performs wonderful actions and undergoes marvellous transformations after the parents have left it as an egg. "'At the right moment' they invincibly obey some sort of mysterious and inflexible prescription Without apprenticeship, they perform the very actions required, and blindly accomplish their destiny" (Legros, p 170)

" . . . Other Cicadellae abound, no less exposed to danger from the Warbler seeking a succulent morsel for his little ones . . . only they do not know how to turn the end of their intestine into a bellows Why not? Because instincts are not to be acquired They are primordial aptitudes, bestowed here and denied there; time cannot awaken them by a slow incubation, nor are they decreed by any similarity of organisation" (*Life of the Grasshopper*, pp. 445-6)

Rightly does one of his biographers refer to "the scrupulous analysis whose lessons, scattered through four thousand pages, teach us more concerning instinct . . than all the most learned treatises and speculations of the philosophers"; though "Fabre has not attempted to define instinct, for it is indefinable" (Legros, p. 169) Darwin thought so too (*Origin*, Ch. VII, para. 2).

Fabre did not propound any really new argument for

vitalism. Though he accepted the idea of a succession of species as revealed by palaeontology and engaged in friendly correspondence with Darwin till the death of the latter, he disagreed with the evolution theory. His views are similar to those of Cuvier and other naturalists of the early nineteenth century concerning the individuality of species and the essential distinction of organisms from non-living matter, and they are open to criticism in details, as may be seen by contrasting them with the modern vitalism of Driesch, Durken, and E S. Russell. But he held these views despite a strongly mechanistic current of thought, supported by new facts and theories in many branches of science, and he fortified them enormously by an unrivalled knowledge of the actual life of the biggest group in the animal kingdom. His place in the history of vitalistic thought is a notable one.

(ii) *A. R. Wallace (English: 1823-1913) and vitalistic attributes of living matter, Man, etc.*

After discussing the vitalism of Kerner, the materialism of Haeckel, and allied matters, Wallace wrote, "Neither 'life' nor 'vital force' nor the unconscious 'cell-soul' are adequate explanations" (*World of Life*, p. 337). It may seem therefore that he should not be reckoned as an exponent of vitalism.

But in this as in many other typical passages (*Darwinism*, pp. 473-8; *World of Life*, Ch. XX; etc ) he was advocating his well-known belief in the necessity for the postulate of a Creative Mind, aided, in Wallace's opinion, by subsidiary discarnate intelligences, as an adequate cause for the whole evolutionary process, and especially for the production of the spiritual, aesthetic, and intellectual powers of Man. With the ultimate necessity for postulating a Creative Mind I entirely agree; but it is our business here to consider what place was given by the independent propounder of evolution through natural selection to vitalistic ideas as proximate explanations for the activities of organism, especially of animals and of Man.

In the passage just quoted he specifically includes "vital" along with mechanical and chemical powers as constituting

that "great variety of forces" for the co-ordination and direction of which a Final Cause is necessary; and he quotes with approval those vitalistic opinions of Kerner, Weissman, and Armstrong to which reference is made below (sec. iv). *The World of Life*, though Wallace was eighty-seven when it was first published, represents in most matters his opinions from the year 1864, when in the *Anthropological Review* he expressed dissent from some of the views expounded by Darwin in the *Origin of Species* and the *Descent of Man* It must be realised that till he was forty, Wallace was as ready to accept mechanistic views as any of his contemporaries (*My Life*, ii, 381-99, Marchant's *Life of Wallace*, i, 243-4; *World of Life*, p. 332); but a few years after his independent conception of organic evolution was published with Darwin's abstract in 1858 he began to change his ideas, primarily because he found that purely mechanical or "natural" forces seemed inadequate to account for the higher mental and spiritual powers of Man, and so came to adopt an attitude which was vitalistic as well as teleological.

A brief comparison of his ideas with those of Darwin goes far to elucidate Wallace's position They agreed, as independent thinkers, that species were evolved from preceding species; and that the principal process by which this was effected was the natural selection of the forms best adapted to the "struggle for life" (*cf Darwinism*, Chs. II and VI; *Origin*, Ch. III). Both stressed the importance of utility (*Origin*, p 181, World's Classics Edition, *Darwinism*, p. 137), though Wallace modified his belief in this later (*e g. World of Life*, p. 320). Both used the familiar arguments from geological and geographical distribution for the evolution of organic forms (*Origin*, Chs 9, 10, 11, 12; *Distribution of Animals; Island Life*), Wallace, with the greater knowledge which arose after Darwin's death in 1882, with greater power (*e.g. Darwinism*, Ch. XIII). They agreed about the great antiquity of Man—in 1859 an unfamiliar doctrine, and about his evolution through natural selection as far as his physical qualities were concerned (*e g Descent of Man ; Natural Selection*, Ch. VIII; *Tropical Nature*, Chs. VII and VIII; *Darwinism*, pp. 445-52, 461). Both believed in sexual selection by males (*Origin*, pp. 80-81; *Darwinism*, pp. 282-3).

And the explanation of the evolution of instincts given in the *Origin* (Ch 7) was accepted by Wallace in his most anti-Darwinian essay on Man (*Darwinism*, Ch XV)

Of particular interest here is their unity on the fundamental problem of the origin of living matter Darwin never altered the famous passages in the *Origin* about the "few forms" or "one primordial form, into which life was breathed by the Creator" (*op cit*, pp 441, 436) Wallace ascribed this creative afflatus in 1889 to "the world of spirit" (*Darwinism*, pp 475-6), and more definitely to the Creator in his later books (*e g World of Life*, pp 392-5). By belief in the special creation of life both admit vitalism to a certain degree, as is shown in II(*c*), ii and iii above

Connected teleologically with this is their common belief, held against Huxley and many less-informed writers, that animals normally enjoy health and happiness according to their capacities, and do not suffer unnecessary pain (*Origin*, pp 72, 441; *Darwinism*, pp. 39-40; *World of Life*, Ch XIX).

There were only four subjects on which they differed, Wallace stated in 1905 (*My Life*, ii, 16-22).

(1) The Origin of Man as an Intellectual and Moral Being

(2) Sexual Selection through Female Choice.

(3) The Distribution of Arctic Plants

(4) Pangenesis, and the Heredity of Acquired Characters.

No. (3) need not detain us here, though it is worth noting that Wallace was in the right according to Hooker and other good judges As regards (4), Wallace's abandonment of pangenesis, which he first hailed with delight (*My Life*, i, p 422), is approved by all biologists.

The first two differences are important In each case Wallace adopted a vitalistic view as opposed to Darwin's teaching which explained the origin of Man's highest faculties and of the song, colours, and decorations of male birds and similar animals as due to the action of natural and sexual selection on small, unexplained variations.

Wallace abandoned Darwin's ideas of female selection in favour of the theory that dominant species possess excess energy which, unless definitely checked by natural selection, manifests itself in brilliant colour, ornament, power of song,

and so forth. The individuals possessing this to the greatest extent would be those possessing the greatest vitality; in virtue of this they would attract or gain the most desirable mates and produce the most vigorous offspring; hence their characteristics would be perpetuated (*World of Life*, pp. 261-2, 271-5). This idea of excessive vitality is itself vitalistic, like the *élan vital* of Bergson, in addition to Wallace's belief that the appreciation of such colour and beauty is due to a divine purpose for mankind. His ideas have lately been independently developed by Dr R Broom, F R S (*Coming of Man*, p 224; *vide* III(*b*), 3, iii)

But Wallace appears most strongly as a vitalist in his views on the origin of the higher faculties of Man,. which are expounded in the last chapter of *Darwinism* Here, after discussing the difficulty—in his opinion, the impossibility—of explaining the origin of mathematical, musical, artistic, and other higher powers in terms of purely natural processes, he propounds "at least three stages in the development of the organic world when some new cause or power must necessarily have come into action" The first is the change from inorganic to organic; "vitality" appears as a new power, inexplicable in terms of chemistry and physics The second is the "introduction of sensation or consciousness, constituting the fundamental distinction between the animal and vegetable kingdoms." The third is the appearance in Man of his higher intellectual and moral faculties (*op. cit*, pp 474-8) These strong beliefs in special properties of living beings are the more noteworthy because, unlike Fabre and Driesch, Wallace was a whole-hearted believer in evolution mainly through natural selection, even admitting that the critics who declared him to be "more Darwinian than Darwin himself" were "not far wrong" (*My Life*, ii, 22).

As up to the end of his ninety years of life Wallace retained the full use of his faculties, wrote vigorously, and read widely, it seems curious that no reference can be found in his books to the work of Driesch, especially as both combine interest in psychic research with the study of biology; *The Science and Philosophy of the Organism* appeared in 1908, two years before *The World of Life* was first published.

Yet Wallace's ideas undoubtedly lead up to the more precise and limited propositions of the talented German.

Both accept evolution, though they differ sharply concerning "Darwinism" Both believe in innate powers of organisms, and appreciate the wonders of development, Driesch concerning general morphogenesis, Wallace concerning the growth of feathers and the metamorphosis of butterflies (*World of Life*, Ch XIV), as inexplicable by purely mechanical conceptions Both believe in special human powers Wallace's bold statements on this subject were criticised by Darwin in the *Descent of Man* and rejected by the majority of biologists, but the work of Driesch, Bergson, and McDougall, together with the possibilities opened out by modern ideas about mutations, has gone far to vindicate his view (*cf* III(*d*), 2, iv) The definiteness of Wallace as compared with the agnosticism of Driesch concerning a unique origin of life has been shown in Part II(*c*) to be a more likely hypothesis Wallace found many arguments for vitalism; Driesch confines himself to particular lines of evidence.

Wallace asked, concerning the nucleus. "What power gave it life? What power organised it? . . . What power *determines* the cell-mass to take this or other well-defined shapes? . . . Who or what *guides* or determines" the new *combinations* chemically, and new *structures* mechanically, *i.e.* as the embryo develops and new tissues and organs are formed? . . . "This orderly process is quite unintelligible without some *directive organising* power constantly at work in or upon every chemical atom or physical molecule of the whole structure" (*World of Life*, p. 347) These are questions to which Driesch replied with his entelechy concept.

Wallace was not a classical scholar, and I cannot find that his biology was influenced by the older authors. It is therefore striking that his vitalistic theory follows that of Aristotle (I(*a*), 1) so closely in its broad lines, allowance being made for the fact that the biogenesis of Wallace marks a definite advance over Aristotle's curious mixture of abiogenesis and a vegetative soul in all organisms.

Wallace was one of the chief promulgators of the still

valid arguments for vitalism summarised in III(*d*), 2, iii and iv, as Biogenesis (No 2), Animal Autonomy (No 7), and Autonomy of Mind (No. 11); and with his idea of surplus vitality originated a strong *indicium* (No. 9) To this list many would add Psychic Research (No. 14) In all these he anticipated McDougall and other moderns. He worked in a mechanistic period and is, in my opinion, easily the greatest "all-round" vitalist since Aristotle.

Appreciations of his great mental powers are quoted in Marchant's biography from Darwin (i, 136, 183, 264, and ii, 1) and Lyell (i, 19-23, 260)

(iii) *Charles Darwin (English: 1809-1882) and his "tendencies"*

It is beyond dispute that Darwin's writings had an enormously strong mechanistic influence (*cf* II(*b*), iv), though he himself was neutral in the vitalism-mechanism controversies So his inclusion among the vitalists is not warranted; yet he cannot be omitted from a summary of "Vitalistic Beliefs after 1859." His idea of the special creation of life was vitalistic (*cf.* II(*c*), and section ii above) And other vitalistic "tendencies" which are patent in the *Origin* demand notice here, for this aspect of the thought of one of the greatest masters of biology is far from familiar even at the present day

In his opinion the "laws of variation" were quite unknown or but dimly seen, and "the laws governing inheritance . . quite unknown" (*Origin*, pp. 11, 12) And, though to-day we know something about the laws of inheritance, thanks to Mendel and his twentieth-century successors, he would be a rash man who claimed any substantial advance over Darwin's ignorance of the causes of variability (*cf. Origin*, pp. 39, 41-2, 78-9) He wrote of "tendencies" to correlation of growth, the laws of which were unknown (*ibid.*, pp. 78-9, 132), though such tendencies were liable, in his opinion, to control by natural selection (*ibid* , p. 130); of the tendency to reversion as well as to variability (*ibid* , pp. 135, 143, 145); of the "innate tendency" of cubs to seek different kinds of prey as illustrating the individual preferences found in a single animal species, with which may be considered his "principle of divergence

of character" (*ibid.*, pp. 82-3, 101-2). Finally, he disclaims any attempt to consider the origin of "the primary mental powers" when discussing the thorny subject of instinct (*ibid*, p. 186).

Driesch's idea that "he might possibly be called even a vitalist" (*Science and Philosophy of the Organism*, p. 173) may seem far-fetched when some of Darwin's later work is considered; but it is not an absurd deduction to draw from the *Origin* which Darwin had five opportunities of revising. Wallace, who knew him very well, has pointed out that Darwin "always adduced the 'laws of Growth with Reproduction' and of 'Inheritance with Variability' as being fundamental facts of nature . . . then as now . . . wholly beyond explanation or even comprehension" (*World of Life*, p. 333).

(iv) *Other biologists of the period.*

Certain German botanists, such as Unger in 1852, Hofmeister, and in 1859, though independently of Darwin and Wallace, C Nagali, arrived at the notion that new species must have arisen from older ones, though they had no such world-convincing explanation to propound as the two Englishmen Nagali is of special interest here. Later, in 1884, his *Mechanical-physiological Theory of Evolution* gladdened the heart of Haeckel by its easy assumption of abiogenesis in the dim past as a sure link in a mechanical theory of life (*cf* II(*c*), iii). Nevertheless, he adhered even in this work to his earlier beliefs against natural selection and in favour of an "inner 'definitely directed variation' " as the cause of the differentiation of species This, Haeckel thought, Weissmann described correctly as "at the bottom merely a 'phyletic vital force'" (*Wonders of Life*, p. 381; *cf.* Sachs, I, Ch. V).

Somewhat similar, but based mainly on zoology, while those of Nagali were on botany, were the ideas of Eimer (*Entstehung der Arten*, 1888 ; trans. Cunningham, 1890). Eimer believed strongly in the inheritance of acquired characters, quoted Lamarck as an almost infallible authority, and opposed the theories of Darwin and Weissmann. His illustrations are often dubious or trivial, and his arguments

seem to me often as feeble as his statements about foreign places, for instance that Muslims display exemplary kindness towards dogs (!), are to an experienced traveller ridiculous (*e g. op cit.*, p. 243). Yet he is regarded as the founder of the theory of orthogenesis (*ibid*, pp. 4, 29, etc); though Professor MacBride points out that Eimer considered the environment as the exciting cause of changes "conditioned by the nature of the animal" (*Eugenics Review*, Vol XIX, p 34)

The modern exposition of orthogenesis is discussed briefly in III(*b*), 3, iii Eimer's views are given here as those of a late nineteenth-century vitalist, for there is no reason to dispute Eimer's claim that orthogenesis clashes with a rigid mechanistic application of natural selection

Lyell adopted Darwin's evolutionary views soon after the publication of the *Origin* (*cf* II(*b*), iv), and, like Darwin, expressed belief in "the Author of Nature" (*Antiquity of Man*, pp 329-30). His ideas also show great affinity with those of Wallace (*op. cit*, pp 392-3), and he expressly opposed materialism thus:—

"So far from having a materialistic tendency, the supposed introduction into the earth at successive geological periods of life—sensation—instinct—the intelligence of the higher mammalia bordering on reason—and lastly the improvable reason of Man himself, presents us with a picture of the ever-increasing dominion of mind over matter" (*ibid*, p. 394, written in 1863).

Asa Gray, another of Darwin's scientific friends, adopted a similar line in his *Natural Selection not inconsistent with Natural Theology*, published in 1861 (quoted by Lyell, *op. cit*, pp. 393-4).

It is difficult to summarise the philosophy of T H. Huxley in a few words. But, though he fought hard for Darwinism, he was no materialist. He maintained Hunter's view that life precedes organisation (*World of Life*, p. 284); and in his *Hume* showed that he realised the shallowness of materialism, for instance "If I were obliged to choose between absolute materialism and absolute idealism, I should feel compelled to accept the latter alternative" (*op cit*, p. 279).

Anton Kerner von Marilaun in his *Natural History of*

*Plants* (trans Oliver, 1894, i 52) wrote as an unabashed vitalist, even as an upholder of the "vital force" scorned in that anti-vitalistic epoch. "More recently . . this vital force was derided and effaced from the list of natural agencies. But by what name shall we now designate that force in nature which is liable to perish whilst the protoplasm suffers no physical alteration and in the absence of any extrinsic cause; and which yet, so long as it is not extinct, causes the protoplasm to move . . . and . . . adapt its movements under external stimulation . . . in the manner which is most expedient?

"Therefore I do not hesitate again to designate as 'vital force' this natural agency, not yet to be identified with any other, whose immediate instrument is the protoplasm, and whose peculiar effects we call life" . . . Its recognition "is not inconsistent with the fact that living bodies may at the same time be subject to other natural forces."

He also wrote of "the appropriate manner in which various functions are distributed among the protoplasts of a cell-community; . . . the purposeful sequence of different operations in the same protoplasm without any change in the external stimuli; the thorough use made of external advantages; the resistance to injurious influences; the punctuality with which all the functions are performed; the periodicity which occurs with the greatest regularity under constant conditions of environment; . . . above all", that these powers are lost by what we call "the death of the protoplasm" (quoted, *World of Life*, pp. 330-1, 345).

Weissmann wrote: "The germ-substance owes its marvellous power of development, not only to its chemico-physical constitution, but to the fact that it consists of many and different kinds of primary constituents, that is, of groups of vital units equipped with the forces of life. . . . The germ-cell . . . must be a fabric made up of many different organisms or units—a microcosm" (quoted *Heredity*, J. Arthur Thomson; *World of Life*, p. 342). He spoke also of a "complex apparatus for the division of the nucleus, of the *purpose* of that division being qualitative as well as quantitative, and of its evident *adaptation* to

the building up of the future body" (quoted, *World of Life*, p 344).

Finally, in 1909, after referring to the properties of carbon as "altogether special", Professor H E. Armstrong, in his Presidential Address to the British Association, spoke of "the complex and extraordinary chemical transformation produced by living plants" and concluded, "The general impression produced by facts such as these is, that directive influences are the paramount influences at work in building up living tissue" (quoted, *World of Life*, p 390)

(v) "*The Tradition*" *according to Driesch.*

A useful summary is given by Driesch in his *History of Vitalism* of some of the writers who maintained "The Tradition" of vitalism during the hey-day of nineteenth-century materialism; but it omits the numerous great naturalists dealt with above, whose work lay outside the only two fields in which Driesch finds definite support for vitalism, *viz.* embryology and human action.

Quotations from von Baer (1792-1876) are used (*op. cit.*, pp 111-12) to show that in 1828 his outlook was rather that of the nature-philosophers than of scientific materialists. His more mature views (*Reden und Abhandlungen*, 2nd edition, 1886; *op. cit*, pp 151-3) were definitely vitalistic, *e g.* "the whole life process is in no way the result of physico-chemical events, but rather controls them." He regarded human actions as autonomous, and rejected parallelism and also Darwinism, with which the mechanistic views of the period were closely connected (*cf.* Singer, *History of Biology*, pp. 304, 469)

Wilhelm His (1831-1904), who specialised on the nervous system and human embryology, and Goette paved the way for Driesch's methods in morphogenesis. Virchow, "the founder of cellular pathology", occasionally expressed dissent from the mechanistic views of most laboratory workers, and J. von Hanstein taught that animal movements and development implied the presence of some organising power, associated with various material combinations or centres. Another acute critic of the natural

selection theory was Albert Wigand (*ibid*, pp. 154-6). Wigand, according to Driesch, attempted no proof of vitalism, but he argued that a "life force" was necessary to account for vital processes which physical forces do not explain. His chief criticism of Darwinism was that the development and structure of such organs as the vertebrate eye could not arise as Darwin, though admitting the great difficulty presented, thought they did (*cf Origin*, Ch VI; and III(*a*), ii below); but that such structures implied teleology of at least the kind that Driesch terms "static" (*Darwinismus*, 1874-7). The last scientific writer quoted by Driesch is Bunge, whose essay in 1890 on *Mechanism and Vitalism* was more remarkable for the general interest it roused at a time when the reaction against Darwinism began to develop than for any decisive arguments in favour of vitalism.

Driesch rightly considers that few philosophers in the period between 1859 and 1895 contributed anything valuable to the vitalistic cause except E von Hartmann (*op cit.*, pp. 158-61); though his metaphysical conception of "life agents" did not demonstrate the inadequacy of mechanism. His *Mechanism and Vitalism in Modern Biology* and *Das Problem des Lebens* belong to a later period O Liebmann opposed the materialism of Darwin's followers and quoted the entelechy of Aristotle with approval Driesch gives a special section to the American biologist and philosopher E. Montgomery, who published a number of articles and books between 1881 and 1907, opposing the machine theory. His influence has been but slight, but he deserves notice for his attempts to solve biological problems by considering mind and body as wholes.

(vi) *Conclusion.*

Vitalism in the early part of the century was uncritical or "dogmatic" (*cf.* II(*a*)), and certain of its features soon perished, as they deserved to, in the rising tide of discovery and criticism, not a little of which was due to vitalists. Belief in vital force as creating energy, never so far as I can ascertain expounded by any prominent vitalists, was destroyed by Liebig and his colleagues (II(*a*), iii). The

extravagances of the nature-plulosophers were eheeked by Cuvier before the great nineteenth-century movements in biology took place. However, belıef ın the special ereation of existing speeıes, as held up to 1859, was eertaınly destroyed by belief ın organıc evolutıon, and the vital force doctrıne of Lıebıg, J. Muller, and other great ehemists of theır day was rejeeted by many scıentısts The period of Wohler, C. Bernard, Joule, Lyell, and Darwın undoubtedly marked a crisıs ın the hıstory of vitalıstıc thought. As Drıeseh says, vitalism eeased to be dogmatıe; ıt now had to fight for ıts validıty ınstead of beıng a postulate for scıentists and the general edueated publıe

Yet the maın elements of the vıtalıstie tradition were maintained and even in certaın cases strengthened before 1895. Abıogenesis, never really eongruent wıth belıef ın the special creation of organısms, but a fundamental tenet of most nature-phılosophers (II(*a*), ıı) and of the materıalıstıc Darwinıans (II(*b*), iv), was finally proved to be wrong by Pasteur and Tyndall, thus the truth of bıogenesıs, a very important argument for vitalısm, was establıshed in the mıddle of the most mechanıstıc perıod of the century (II(*c*)). The characterıstıc features of Arıstotle's vıtalısm were enunciated *de novo* by Wallace (sectıon ıi above), who elaborated a vıtalıstıc system capable of blendıng both wıth the natural selectıon theory and the opposıte vıew of Drıeseh (*cf.* III(*d*), 2, v) The epigenetıc doetrıne of Harvey and C. F. Wolff was developed by Meckel, von Baer, and succeedıng embryologısts Singer and others consider, ın contradıstinetıon to Drieseh, that the vıews of Stahl and J. Muller are represented to-day in bıologıeal thought; and most of the leadıng ehemısts eontınued to be vıtalısts Muller's doetrıne of nervous energıes and the epistemology begun quıetly by Ferrıer in 1854 were to lead to one of the most powerful modern erıticisms of meehanism (*cf.* III(*b*), 1, ıv). The vitalıstic view of lıvıng organisms was maintained by Wıgand and Kerner, who continued the employment of the term "vital foree", and reinforced by Fabre, von Baer, and others Eımer's orthogenesıs was antı-mechanıstic. Darwin, Nagalı, and others were not so antı-vitalıstıc as they were painted by Haeekel and his school. The way

was kept open for the new vitalism to operate as soon as biological research flowed again over the "world of life" instead of being mainly confined to the narrow channels which the mechanist school takes for Newton's ocean of truth.

# PART III

## RECENT DEVELOPMENTS: 1895-1938

# III(*a*.)—DRIESCH (GERMAN: 1867-    ) AND ENTELECHY

". . the honest and rigorous following up of the argument which leads us to 'materialism' inevitably carries us beyond it"

T. H. Huxley, *Hume*, pp 251, 279.

(i) *Historical.*

A useful summary of "neo-vitalism" or the revival of vitalism which occurred at the close of the nineteenth century is given by Dr Hans Driesch in his *History and Theory of Vitalism* (pp. 170-83). Though incomplete, it includes a valuable account of the evolution of his own biological philosophy.

He attributes this revival primarily to the "reappearance of experimental morphology . . . represented by W. Roux"; although Roux himself was not explicitly a vitalist, he developed the "mechanics of development" in which Driesch found new experimental support for considering that the vitalistic view of life is the true one

Tribute is paid to Mach as overthrowing the mechanistic metaphysic which nineteenth-century scientists accepted without question; though Mach's ideas did not receive immediate acceptance they prepared the way for modern epistemology (*cf.* III(*b*), 1, and III(*c*), 1, below)

In 1890 and 1894 Gustav Wolff published his books which criticised mechanistic Darwinism and argued, as F. Ehrhardt did, for teleology He tried to demonstrate "primary finality" by a single experiment; the lens of the eye of a newt was removed and a new lens was developed from the iris, whereas normally the lens arises from the skin or ectoderm. Driesch doubts whether this single experiment proves "dynamic teleology", but he was profoundly influenced by G. Wolff's demonstration of the theoretical significance of teleology and by his general vitalistic outlook, shown *inter alia* by his acceptance of the autonomy of psychic life (*Beitrage z. Lehre v. den Sprachstorungen*, 1902). Driesch was also

indebted to Wigand and Paul du Bois-Reymond, the latter of whom argued for the independence of the various divisions of science, including biology (*op cit*, p. 176).

Driesch then gives an interesting summary of his own ideas and biological publications up to 1905 His *Biology as an Independent Science*, first published in 1893. vacillated, he says, between recognition of a creative principle and a mechanistic teleology, and the latter theory dominated his *Analytische Theorie der organischen Entwicklung* in 1894 In 1896, in *The Machine Theory of Life*, he repeated the substance of these two books, but also outlined the conception of dynamic teleology which plays such an important part in his later works. His experimental work in embryology, combined with logical analysis of the concepts of regulation and action, had led him to belief in Vitalism; in 1899 in *Die Lokalisation Morphogenetischer Vorgange* he expressed his beliefs clearly, showing that some life processes are autonomous or dynamically teleological This was followed in 1901 by *Die organischen Regulationen*, in 1903 by *Die Seele als elementarer Naturfaktor*, and in 1904 by *Naturbegriffe und Natururteile.* The first of these added a new proof from the regulative working of the living body as a whole; the second discussed human action as an argument for vitalism; the third gave a methodological treatment of his vitalistic system. In 1907-1908 his Gifford Lectures gave its most complete and best known formulation.

As regards the *History and Theory of Vitalism*, the English translation, by C K Ogden, is dated 1914, but the book was first published in German in 1905, and the Historical Part remains much as it was first written, though the Theoretical Part was completely rewritten for the English edition. This gives Driesch's system of vitalism in a deductive way, whereas the method followed in the Gifford Lectures is inductive. As a short summary it may be preferred by some students to the longer work, especially by those whose studies have been logical rather than biological. At all events, it is helpful to have an alternative treatment for the best known modern presentation of the vitalistic outlook.

The historical part has been frequently used in the compilation of this book. But for the twentieth century it is naturally almost silent, and it must necessarily remain in-

complete as the historian cannot be expected to analyse or appraise his own important contribution to vitalistic history. Further, its attitude in certain passages does not represent the author's more mature views

Still the book remains an indispensable work for the historical study of vitalism; but the following points call for critical comment. It has no index. Galen is not even mentioned nor the "spirits" doctrine which he established; nor are Fabre and other distinguished vitalists, who are discussed in my Part II(*d*). Attention is confined too closely to embryology as an argument for vitalism; co-ordinated movements of animals and other phenomena which suggest vitalism are barely alluded to and biogenesis not at all. In philosophy, apart from an unnecessarily lengthy account of the American Montgomery, non-German writers receive little more than bare mention, a notable illustration of this being the scanty allusion to Bergson. Kant, on the other hand, is given a disproportionate amount of space. Driesch's sweeping censures of the teaching of Darwin's immediate followers do not here take sufficient account of Darwin's own cautious attitude, and no mention is made of A. R. Wallace! The bitterly sarcastic references to "Darwinism" are doubtless due to the excessive veneration given to "*Darwinismus*" in Germany in the later part of the nineteenth century by Haeckel and others. "For Driesch began as a disciple of Haeckel but through the influence of G. Wolff and W. Roux came to support a dynamic vitalism" (E.B., 7, 662; *cf.* Part III(*c*), 2, ii, below).

(ii) "*The Science and Philosophy of the Organism*"; *a critical examination thereof.*

This work is divided into numerous portions, the biggest being most unsuitably termed "Sections." Of these there are two, corresponding to the two parts of the title:

A. The Chief Results of Analytical Biology;

B. The Philosophy of the Organism.

It is unfortunate that A, B, C, and D are also used for the subdivisions of the "Parts" of each "Section."

In the "Introduction" (pp. 1-10) Driesch explains that

the subject matter of the biological portion, "Section A", can be approached on a basis of naive realism. But later he states that a critical, even a solipsistic, basis is also possible (*ibid*, p 326).

He then gives some much-needed warnings about "Certain Characteristics of Biology as a Science." It can never be too frequently pointed out that, whereas the investigator of inorganic science can repeat experiments at will, "the biologist is dependent on the specificity of living objects as they occur in nature." This means that biology depends largely upon observation of phenomena which are not repeatable at will by the scientist, and that experiments performable upon certain organisms or parts of organisms cannot be performed upon other organisms For instance, to quote one of Driesch's illustrations, it is possible to detach the membrane from certain young embryos; but when this is done with other species the embryo dies at once and cannot be watched or experimented upon

He also deals firmly with the suggestion that physiology is more scientific, because more rational, than morphology, a view blatantly expressed in J Needham's *Sceptical Biologist* (*e g* pp 135, 252; *cf* my III(*b*), 3, ii) Driesch, very properly, will have none of this elimination of the greater part of biology to suit the narrowed outlook of specialists in biochemistry and similar subdivisions of biology. His declaration that "the experiment (*e g.* in physiology) . . . possesses no kind of logical superiority over pure description at all" (*ibid.*, p 3) affirms the essential autonomy of biology as a science which is not a miserable appendage to chemistry and physics.

He then outlines the plan of the book which is to deal with three fundamental "characters" of living bodies—form, metabolism, movement. Under the study of form (morphology) he also includes development, which he always terms "morphogenesis." By linking these two together Driesch coordinates the phenomena of growth in young organisms with the secondary growths (restitutions) which occur in many adults. Finally, he draws attention to the small part played by evolution in his treatment of organisms, because, he considers, so little is really known about it (*cf.* 147 *seq.* below).

Driesch's "Section A" forms more than two-thirds of the whole book and has most interest for biologists, for it contains not only an exposition of Driesch's views on vitalism but an account of the experimental work upon which they are based It is in three parts:

I. The Individual Organism with regard to Form and Metabolism.
II. Systematics and History.
III Organic Movements.

The final heading in the Table of Contents for Section A is misleading; its last two pages do not give the "Conclusions" to be derived from a study of the whole "Section," but only some considerations connected with "Organic Movements." The real "Conclusions" to be drawn from Section A are given in "IB 3, ID," and "III 3d 8," in which Driesch summarises his three proofs for entelechy.

"Part IA" gives an introductory account of cytology and the early development of *Echinus* Epigenesis is admitted as true; there is a "production of visible manifoldness" in embryology.

"Part IB, Experimental and Theoretical Morphogenesis", is in many ways the most important part of the book. In its first chapter he describes Weissmann's theory of development as very similar to the "evolutio" doctrine of the eighteenth century, as according to each the egg contains the rudiments of the structure of the chick, though not of course in a visible form; so he calls Weissmann's theory "dogmatic" (*ibid*, pp. 34-6). Wilhelm Roux, in *Die Bedeutung der Kernteilungsfiguren* (1883), advocated a theory similar in many ways to Weissmann's; but the experimental work done by Roux to support it reintroduced a new era of "scientific experiment in morphology." Roux's classical experiment is described; he killed one of the first two blastomeres in a frog's egg and found that the survivor produced a half-embryo. This seemed to be a definite proof of the belief of Weissmann and of Roux that ontogeny is based upon a complicated though invisible structure in the egg (*ibid.*, pp. 37-8). Roux's experiment was made public in 1888.

Driesch then describes the crucial experimental study
embryology or morphogenesis undertaken by himself an
confirmed and extended by T H. Morgan and other bio
logists; and apart from its resulting in Driesch's theory
entelechy the process is of supreme importance in showin
how little value a single experiment has in biology, or i
allied fields of research such as, for example, education, even
if it is properly performed and correctly interpreted

Driesch selected the sea-urchin (*Echinus*) for practical reasons and was able to repeat Roux's work on the ovum of the frog. He succeeded in destroying one of the first two blastomeres and in separating them so that development of only half the original egg-substance proceeded. The first stages resembled those of Roux's experiment and Driesch fully e[illegible] to get a half-embryo in the later stages too. Actually [illegible] a whole blastula, then a whole gastrula, followed by a whole pluteus-larva; only they were half the usual size. The half material had become rearranged and carried out a complete development; which was exactly the reverse of what Roux had found in the frog! (*ibid*, pp. 38-40).

This showed that the first nuclear division did not separate either the nuclear germ-plasm or the cytoplasm of the egg into two fundamentally dissimilar halves and so destroyed the basis for the theories of Weissmann and W Roux.

But Driesch was not content with a single group of experiments. He also showed that even one from the first *four* blastomeres could produce a small but perfect larva and that three of these four cells would produce a perfectly normal organism. He then discovered that even though the cleavage stages were seriously altered the resulting organism was normal. O Hertwig, E. B. Wilson, T H. Morgan and Spemann repeated his experiments successfully with other types of animals, Spemann making an interesting modification. Finally Driesch and Morgan succeeded in developing single giant organisms from two fused blastulas, the former with *Echinus*, the latter with the newt (*ibid.*, pp. 42-3).

From these experiments Driesch deduces that nuclear divisions—at least during the first, cleavage, stages—have no bearing on organogenesis; and in view of the multiplicity

and variety of the experiments the correctness of his view must be admitted.

Yet, he urges, there must be somewhere in the egg a factor which controls the "general orientation and symmetry" of the developing embryo. This he concluded in 1893 (*Mitteil, Neapel*, 1893) must be due to some delicate structure in the protoplasm of the egg. In Echinoderms it must be capable of quick rearrangement after disturbance and so not observable, in the frog's egg Roux's work indicated that it was permanent and so might be discoverable. To test this hypothesis Driesch and T. H. Morgan experimented in 1895 on the eggs of ctenophores (*Arch. Entw. Mech.* 2, 1895): they repeated experiments made earlier by Chun and confirmed his results except in *Beroe*. Then they cut away some protoplasm of the egg before cleavage began, without damaging the nucleus. When this cut was made at the side the resulting larva showed the same defects as a larva developed from one of the two first blastomeres (*ibid.*, p. 44). Thus the hypothesis of the morphogenetic importance of the egg protoplasm was proved. But he considers it responsible only for "the general symmetry" of germs and of their isolated parts (*ibid.*, p. 46).

O. Schultze then experimented on frogs' eggs and succeeded in getting two small whole embryos from the two-cell stage by slightly pressing the latter and turning it over; while Morgan got whole or half development from one blastomere after killing its fellow according as it was undisturbed or turned over. From these facts Driesch concludes that whole development from a single blastomere in the frog's egg depends on rearrangement of the protoplasm caused by turning it over.

Such regulation he terms facultative; in *Echinus* this regulation is obligatory. Newts, too, belong to the obligatory class though closely allied systematically to frogs. Egg protoplasm then must have an intimate structure which may be described as polar-bilateral. The development is whole, despite "disturbances", if the intimate structure became whole first; it is half or quarter if only a half or a quarter of this "intimate" protoplasmic structure is present in the separated blastomere (*ibid.*, p. 45).

Another problem is that connected with cell-lineage. In

*Nereis* (studied by E. B. Wilson) and various other animals the origin of certain organs is traceable to individual cleavage-cells On Driesch's hypothesis this must be due to peculiarities in their protoplasm, and he propounded this view in 1894 It was confirmed in 1896 by Crampton (*Arch. Entw. Mech.* 3, 1896) by experiments on a mollusc, and then by Wilson. Removal of a special part of the embryo in the two-cell stage resulted in absence of mesenchyme in otherwise complete larvae; and the *protoplasm removed contained no nucleus.*

This terminates what Driesch calls the first period of "the new science of physiology of form", and he summarises his conclusions as follows.

(1) The theory of "qualitatively unequal nuclear division" is disproved, and Weissmann's theory with it

(2) Epigenesis or "production of manifoldness" is generally true.

(3) But in a restricted sense there is also preformation. This is confined to the protoplasm and is of two kinds: first, a polar and bilateral structure, demonstrated in many and to be postulated for all germs; secondly, in some organisms a specificity of egg protoplasm, so that certain parts produce certain organs.

(4) Differences of regulability depend only on the physical consistency of the protoplasm.

(5) Much remains to be discovered about specific organisation, but it is unlikely that two very different kinds of morphogenesis exist side by side in the same classes of animals (*ibid*, pp. 48-9)

These results seem to oppose the generally accepted theory of geneticists that inheritance is equal from both parents. Equality between the gametes is usually confined to the nuclei; but specificity of the egg protoplasm indicates that the female parent has a greater share in ontogeny than the male. This, however, occurs in its very early stages only.

Driesch then proceeds to his "Analytical Theory of Morphogenesis" ("Chapter 2 of IB"), in which he distinguishes six different problems, indicated by Greek letters.

"α The Distribution of Morphogenetic Potencies." He distinguishes the *prospective value* of any element in an

embryo, which is what it actually develops into, from its *prospective potency*, which is what it may develop into These may be identical, but are not necessarily so, as the experiments mentioned show; *e g.* one of the four blastomeres of *Echinus* usually provides about a fourth of the larva—prospective value; but under special conditions it *may* produce a whole, though small, larva—prospective potency Prospective potency is the important matter for study from a philosophical point of view.

An *Echinus* blastula, comprising about one thousand cells, if bisected in any plane passing through or near its polar axis, may form two fully developed organisms; so that "the prospective *value* of any blastula cell is a function of its position in the whole" This Driesch found to be true, too, of the ectoderm and endoderm cells in the *Echinus* gastrula; but while ectoderm and endoderm are each equipotential, they are of different potencies compared with one another (*ibid*, p 56) Such potencies as these, noted during embryonic development, Driesch terms primary.

But the course of development varies in different animals. In the early embryos of mollusca, for instance, there is no equal distribution of potencies, the cleavage cells forming a "mosaic" as regards their morphogenetic potentialities. Reasoning from experimental work done by Conklin and E. B Wilson and adopting the induction of the latter (*Jour. Exp. Zool*, *I*, *1904*) Driesch argues that ontogeny really begins before fertilisation, with maturation in fact He concludes that all eggs have a stage in which all parts of the protoplasm are equipotential, but that the change to a condition of specificity occurs at different times in different organisms; in some, such as *Echinus*, only when the germ layers are formed; in others, such as mollusca and nemertines, during maturation (*ibid*, p. 60).

So far, however, no definition of prospective potency has been reached.

"*β*. The 'Means' of Morphogenesis" (pp. 62-7). By this phrase Driesch understands the circumstances which are necessary for complete morphogenesis, and he divides them into internal and external

Internal "means" or conditions include the elementary

functions of the organism, such as secretion, cell migration, division and growth, and physical states such as surface tension and osmotic pressure Here the conflict between vitalism and mechanism occurs in an acute form. Driesch expresses high appreciation of biochemical and biophysical investigation, but he claims that however thoroughly any of these "conditions" may be understood morphogenesis is not thereby explained, because what may seem to be due to mechanical pressure may be really caused by active growth, and so on; and, anyhow, these processes do not constitute life but are merely used by it

Finally he points out that morphogenetic regulation affects the form of the whole organism not the form or size of individual cells, the latter depending upon the mass of the chromatin according to Boveri Thus cells appear as the brick-like material which the organism uses to build up its organs (*cf* Durken, III(*b*), 3, v)

Little need be said about External Means· Driesch fully recognises that organisms depend upon suitable temperatures, the presence of oxygen, and so forth, for their existence and development Tribute is paid to Herbst for his investigations on the effect of calcium and other chemical components of sea-water upon the development of *Echinus* (*Arch Ents Mech*, 17, 1904) His discovery that the addition of lithium and other substances causes abnormalities in development is noted, also the strange fact that the inverted "lithium-larva" is produced in echinids but not in asterids (*Zeitschr. wiss. Zool*, 55, 1892; *Mitt Neapel*, II, 1893).

"$\gamma$. The Formative Causes or Stimuli" (pp 67-74) Driesch admits difficulties in the concept of "cause" but concludes that its use is nevertheless legitimate As regards embryology, he regards prospective potency as the immediate cause of every specification affecting single organogenetic processes; but he links with this "that occurrence on which depends its localisation", adding that there is no quantitative correspondence between "cause" and morphogenetic effect.

Heliotropism and geotropism are spoken of as physical functions of plants (*ibid*, p 70) The growth of galls is an example of the combined effect of external formative stimuli (insect attacks) and the potency of the plant to respond in a

particular way In *Bonellia* external conditions affect sex determination (*ibid*, p. 73) Among examples of "internal formative stimuli" quoted is the first one discovered by Herbst, that is, the dependence of the arms of the pluteus larva of *Echinus* upon the internal skeleton

An "enormous variety of formative dependencies" in the embryos of *Triton* (newt) has been discovered by Spemann and his school Here no influence is exerted by the nervous system, as happens in some other cases, and Driesch compares the physiological effect of hormones with what he thinks is an unknown morphogenetic influence The important point here is that the *Triton* embryo is highly equipotential

"δ. Embryology and Time" (pp 75-6). "Every single embryological process occupies a specific temporal position within a well-ordered sequence of events"; and unless it occurs at the right time it does not occur at all. This is rightly described as the most important matter in morphogenesis from the temporal point of view, and it is based on numerous experiments

"ε. Morphogenetic Harmonics" (pp. 76-9). Organogenesis occurs "in separate lines . . . which may start from a common root, but which are absolutely independent of one another in their manner of differentiation " Thus, Driesch explains, the development of a part A is not dependent on that of concurrent parts B, C, D; but it may have been "formatively dependent" on previously formed parts E and F and there may be various formative actions among the constituents of A itself. But the resulting organism is a whole in organisation and function The rest of this section is obscure.

"ζ. Restitutions" (pp 79-84). (*Cf.* Driesch, *Die organischen Regulationen*, 1901; T. H Morgan, *Regeneration*, 1901). The five preceding sections deal with ontogeny, the development of the individual from the germ or egg. But morphogenesis also occurs in the adult in many animal phyla, and analytical morphogenesis must discover the "specific and real stimulus" which evokes processes of restoration or restitution.

Primary or ontogenetic processes may involve primary regulation or restitution, at least under experimental con-

ditions, as when part of a blastula builds up an entire organism. Secondary restitutions are those which occur whenever a disturbance of organisation is rectified by abnormal processes, they reveal potencies which are latent during ontogeny; they occur in the adult, or partly adult, organism.

That type which Driesch terms "*re-differentiation*" is discussed in the next chapter; in this all the elements of the organisation take part.

*Regeneration* is restitution on the site of the wound or disturbance. *Adventitious processes* occur at some distance from the wound. These two processes Driesch terms "complex."

Fourthly, *compensatory hypertrophy* is the abnormal development of one organ to compensate for the loss of another; *e.g.* a kidney may become larger when its fellow is removed, a leaf may be developed from a scale, and so on.

Finally, at least in plants, a change of the directive irritability, *e.g.* "geotropism . . . in certain parts may . . . restore other more important parts."

Driesch then discusses the nature of the stimulus which calls forth restitution (*cf. Stimuli of Restitutions*, Internat. Zool. Congress, Boston, 1907). That the wound itself is not this stimulus is shown by restitutions that occur not at the wound, and the removal of an obstacle does not explain why a particular growth, that which is needed, occurs. The "Auslosung" theory of compensatory development will cover certain cases, such as those mentioned under compensatory hypertrophy, but not others in which quite new formations or the regeneration of old ones restore damaged parts. Food assists restitution but does not "cause" it, as Morgan has shown that true regeneration occurs whether the animal is fed or not. Something may be due to the effect of possible additions, similar to hormones, added to the body fluids by each part, so that their absence gives a stimulus to the body, but this, in Driesch's opinion, does not account for the complicated nature of a restitution. So the problem of the stimuli which evoke restitutions is not solved at present; but it points to the idea of *wholeness* as an important one for biology (*ibid.*, p. 84).

"Chapter 3. Morphogenetic Localisation" (pp. 85-109). Its importance is indicated by the sub-heading, "First

Proof of the Autonomy of Life" Driesch starts from the three elementary concepts previously discussed—prospective potency, means (or conditions) and formative stimulus, and asks if they are sufficient to explain every morphogenetic event. Thence arises the further question: Does a specific formative stimulus account for the localisation of every morphogenetic event?

From cambium a whole branch or root may arise, so its potency is *complex* But he is hardly correct in saying that a single cambium cell can give rise to a root or branch (*ibid.*, p. 87). In the embryonic layers mentioned the function of each single part depends on its position, this Driesch terms a *singular equipotential system* But there is a harmonious or correlated growth, so that the most suitable name is *harmonious-equipotential* system (*ibid*, p 88).

Driesch then analyses "the fate of any element of our system" at considerable length, expressing the prospective value of an element *x* as a function of various factors. *One* factor is the size of the part *s*. Another is its position in the part or system cut out of a whole embryo, which he represents by "*l*" The formula then becomes *f* (*s l* . . .) where *s* and *l* are independent variables. So far it is easy to follow him. It can also be agreed that there is a "certain factor" at work in every case of development, normal or experimental. But his statement that this is *not* a variable but is the same in all cases (*ibid*, p 90), is at this stage an unjustifiable assumption. This can be added to the formula as "*E*," it seems to me, whether it is variable or not In either case the completed formula can be written as *f* (*s.l E*). This mathematical equation has, of course, no special validity *qua* equation or formula, and its introduction seems to me quite unnecessary

A very interesting account of experimental work upon restitution in the hydroid *Tubularia* and the ascidian *Clavellina* follows, with reference to similar experiments upon *Hydra*, *Planaria*, *Stentor*, and other animals (*ibid.*, pp. 92-6). Parts of these animals are able to regenerate the entire organism and are admirable examples of *harmonious-equipotential* systems. The new head in *Tubularia* is not restored by ordinary regeneration but by the combined work of many

parts of the stem The biological student will understand what Driesch means by this term much better from a study of these experiments than from his rather difficult abstract analytical treatment

He then returns to abstract analysis of the factor *E* (*ibid*, pp 96-105) On p 97 he explains *E* as "the prospective potency, with special regard to the proportions embraced by it", and so a constant (of a very indeterminate kind, in my opinion) for any given layer or organ, such as a part of the branchial apparatus of *Clavellina.* He then sets out to show that *E* is not explicable by (i) Means or Formative Stimuli, (ii) Chemical Theory, (iii) any Machine Theory (*ibid.*, pp 97-104)

Means have been shown above to have no value or power for localising organogenesis. In a footnote (*ibid*, p 97), he shows that, if external conditions are thought to have this power, the result would be normal development at the cut ends or edges and non-development in the unharmed middle. And no such stimulus comes to the fragment from without as it does when a lens is formed owing to the optic vesicle touching the skin

As regards chemistry, the first theory dealt with is that a very complex compound, which may be the basis of development and inheritance, may "direct" morphogenesis by its disintegration Driesch's first objection is that if restitution or regeneration is caused by this hypothetical disintegration, the rebuilding of the compound in the restored part or organ is unaccounted for (*ibid*, p. 99)

He then makes another criticism, chemical disintegration cannot account for the differentiation found in harmonious potential systems which are not affected by localising causes. It is not the chemical composition of, say, bone that is in question but the required arrangement or form of the bones which appear in the right place and shape in regenerated limbs and other organs As he sums up, specificity of form does not go hand-in-hand with specificity of chemical composition and therefore cannot depend upon it nor upon molecular arrangement (*ibid*, pp. 99-101) This argument is thoroughly sound.

The machine suggestion is the most plausible non-vitalistic

explanation for development; but it must cover abnormal morphogenesis and restitutions as well as normal developmental processes. And as machine must here be understood in a "most general sense" and must include the existence of "chemical constituents", Driesch thinks that such a highly complex machine might account for undisturbed development, if the removal of parts of the systems of the embryo lead to fragmental development, such as Roux's experiment with the frog embryo indicated

But his experiments prove that parts, his harmonious-equipotential systems, actually behave quite differently to what this theory demands; and this is the *crux* of his argument. If the machine theory were true, any moderately sized part of the machine, taken anywhere in it, must be able to act like the entire machine; so that all parts of the system are at the same time constituents of different machines (*ibid.*, p. 103) And these different machines may be of various sizes and may overlap one another But "a machine . . . cannot remain itself if you remove parts of it or if you re-arrange its parts at will"

That is, the experiments begun by Driesch and continued by himself and others on mutilated embryos or adult organisms show that morphogenesis is not explicable on mechanistic lines (*ibid*, p. 104). I see no escape from this argument, based upon so many and so various experiments and observations. And it is accepted by Wightman, who, however, cannot accept Driesch's entelechy (*Science and Monism*, pp. 349-51; *cf.* p. 156 below) Objections by modern mechanists such as J Needham (*cf* III(*b*), 3, ii) are based on rejection of the concept of organism; but for most biologists this concept is fundamental (*cf.* III(*d*), 1; III(*b*), 3, v; etc ), so such objections, being based on *a priori* assumptions, are worthless.

Morphogenesis then is not "a specialised arrangement of inorganic events", Driesch very properly argues; and so biology does not comprise only applied physics and chemistry. "Life is something apart, and biology is an independent science" (*ibid.*, p. 105).

He adds that his results, as so far expressed, are negative, and that the evidence is obtained *per exclusionem*, that is, by

discussing all other hypotheses and proving that they are incorrect, or at least inadequate, so that the remaining one must be accepted as correct But, as he says, that is the normal scientific method, which leads to "something new and elemental" being introduced whenever new discoveries make it necessary (*ibid*, p 105) The idea of radiation is a recent example of this in the realm of physics. there are many others.

What he has proved then may be termed vitalism; at least he has supplied a proof (his first) of the autonomy of life. From Aristotle he adopts the term "entelechy" for the vitalistic or autonomous factor which is essential for morphogenesis.

This chapter closes with a claim for the universality of scientific concepts in biology as in other sciences; which here means that if certain embryos are found to behave in a certain way it is reasonable to suppose that similar ones would do the same. Provided that a sufficient number of experiments and observations are made to prevent the scientist from drawing rash conclusions from one or a few aberrant individual cases this hypothesis must be admitted.

"Chapter 4" discusses certain other features of morphogenesis which indicate autonomy in living things

In the section "On Certain Combined Types of Morphogenetic Systems" Driesch points out that the harmonious-equipotential system is not unique. There is also the *complex-equipotential* system, in which any one of several constituent elements may produce a whole organism as in the 2, 4 or 8-celled cleavage stage of *Echinus.* But as 2 or 3 of its four cells or 3, 4, 5, 6 or 7 of its eight cells may also produce a whole organism, the *Echinus* embryo is also a *harmonious-equipotential* system *Planaria* is another example

To these two systems and their combination Driesch adds the *complex-harmonious system,* as seen in the tail or leg of the newt and the body of *Planaria,* because these have been shown by Godlewski and Weiss to be capable not merely of regenerating a tail, foot or head to reproduce a completely severed member, but of forming a new member when the section is only partial. He also mentions briefly the "super-regeneration" which sometimes occurs after a com-

plicated wound is inflicted But he claims nothing new from these

The *reciprocity of harmony* seen in the correlated development of new tissues from the various elements of a cross-section is an important feature of morphogenesis. *Mirror* and *transgressing* equi-potentialities (*ibid.*, p. 113) indicate an enlargement of ordinary harmonious equipotentiality, but do not need special discussion here.

Restitutions of the second order have been found in *Tubularia* by Driesch (*Arch Entw Mech*, 5, 1897). While normal restitution was proceeding he removed the terminal ring of developing tentacles, after this disturbance the restitution itself was regulated. Similar occurrences have been discovered in *Planaria* (Morgan), *Clavellina* and other cases (*ibid*, pp 113, 116). Now in *Tubularia* this secondary regulation may occur in three different ways, yet the end is the same Yet, though many people might see in this *equifinality of restitutions* another proof for vitalism, Driesch will not count it as one, but includes it in his first two proofs.

He concludes, however, and rightly, by saying how inadequate are (ordinary) scientific conceptions when confronted with such phenomena of life finally mentioning the great extent to which regeneration may be repeated (*ibid*, p. 117).

"Part I, C" deals with "Adaptation" (pp 118-141). As Driesch does not claim any positive results from its consideration, it need not be discussed at great length. A few definitions may, however, be noted.

Regulation is the occurrence or group of occurrences which takes place after disturbance of the normal organisation or state of functioning and leads to their reappearance; the former processes are restitutions, the latter adaptations.

Mimicry, protective coloration, and so forth show adaptedness, not adaptations, in Driesch's phraseology.

Primary regulation is that which serves to maintain normal organisation or functioning. Secondary regulation serves to re-establish normality after disturbance "*along lines outside the realm of so-called normality.*" He considers this distinction a very important one (*ibid.*, p. 121).

He is prepared to grant that many processes in the living body which appear to be against ordinary laws of inorganic

science are really explicable by physics and chemistry in view of the complexity of the organism (*ibid.*, p. 120)

But after a critical discussion of "Physiological Adaptation" Driesch concludes that secondary regulation is realised "beyond doubt" in immunity against diseases. Cells of the organism, such as the white blood corpuscles, attack and kill noxious bacteria; membranes are formed to isolate trichinae and some kinds of bacteria; but the chief examples of the reaction of organisms against diseases and organic poisons are the "anti-bodies" formed by warm-blooded vertebrates. There is not merely the production of sufficient antitoxin to neutralise the toxin actually present, but the production of a surplus to guard against future attacks (*ibid.*, pp 136-8).

Yet he counts this only as a strong *indicium* that the organism is more than a machine This is a good example of the caution typical of his argument. Many people, unless they refuse to admit the existence of organisms altogether, would see in these wonderful reactions of the organism a definite proof for vitalism.

"Part I, D, Inheritance" (pp 142-54) is important, as Driesch finds here his second proof of the autonomy of life.

Earlier (*ibid*, pp 34-5) he criticised Weissmann's germ-plasm theory as dogmatic and insufficient as an explanation for the facts of morphogenesis Here he expresses pleasure because the continuity of some material as "one of the foundations of inheritance" has been clearly stated. But he points out that, owing to metabolism, this does not imply that the *same* community of atoms "or what you will" persists throughout many generations. Material *sameness* is true only of the matter connecting two generations; as regards all generations the sameness is of one quality, not of identical matter (*ibid*, p. 143; *cf.* p 117).

Inheritance is then considered with reference to complex-equipotential systems (*cf.* Driesch, *Organ. Regul.*, 1901): in these, complex acts, consisting of a manifoldness in space and time, can be performed by one of several single elements. Such a system is the cambium of flowering plants, even if each of its elements could restore only root or branch. Better examples are seen in liverworts and the leaf of the begonia, in which single cells can reproduce entire plants

(*ibid.*, p. 145). In animals complex morphogenetic systems are seen in the legs of newts, etc.: the section may be made anywhere yet a complex organ—leg and foot—is regenerated. But in many annelids or in *Clavellina* there are true complex-potential systems. The section develops the posterior half on one part and the anterior half on the other; and the section can be made in various places. Finally, such systems are seen in the sexual organ, notably the ovary. Every cell (ovum) formed from the ovary can reproduce a whole organism, in some animals by parthenogenesis.

Hence, by an argument full of details but simple in outline, Driesch deduces a second proof for "vitalism" (pp. 147-9). Put briefly, it is that no machine conception, however far it is pushed, can account for the origin from an ovary of single cells capable of reproducing a whole organism and that an almost exact copy of the parent, however large or differentiated it may be. If a machine cannot account for this excessively common but nevertheless marvellous occurrence something else must; and to this principle actuating complex-equipotential systems he wisely gives the same name as to that which actuates harmonious-equipotential systems—entelechy.

This second proof is independent of the first proof because it is based on a different set of biological phenomena; though it has a bearing on the validity of the first (*op. cit.*, pp. 149-50).

Is his argument sound? Put in other words, it means that inheritance and the beginning of ontogeny cannot be explained mechanistically. This is surely valid reasoning, except for those to whom the concept of organisms is meaningless.

The remainder of this chapter outlines the Mendelian and gene theories, indicates his acceptance of both, and concludes by stating that genes are means for inheritance and no more, and that "all *order* in morphogenesis is exclusively due to entelechy" (*op. cit.*, p. 154).

In his summary of "Section A, Part I" (*ibid.*, pp. 155-6), devoted to the study of individual morphogenesis, Driesch is succinct and clear. This has, he claims, proved an autonomy of life phenomena in some departments, and probably in all

of them. There cannot be a machine in the cell from which the individual originates because this cell results from a long series of divisions, affecting nucleus and cytoplasm, and a machine cannot be divided and yet remain what it was. I would add that the alternative way of thinking that a machine can generate a lot of other, little, machines is equally absurd This is his second proof from complex-equipotential systems.

Nor can a harmonious-equipotential system be explained on a machine theory because its development proceeds normally even if its parts are rearranged or partly removed, and no machine can function under such disturbances This is his first proof of the autonomy of life

He has not claimed other phenomena, such as regeneration, as proofs; he considers regeneration as only another phase of complex-equipotential systems.

"Section A, Part II" deals with Systematics and History (pp. 157-84)

Biological systematics, Driesch claims, is nothing but classification, although in the various "natural" systems it has been carried to a high degree But though we have excellent classes, orders, and sub-divisions thereof we cannot say why there must be just these groups and no others, nor why these should be as they are

The two most important concepts gained from biological classifications are those of the "type", due to Cuvier and Goethe, and correlation of parts Embryos show type characteristics better than adults; but even in them specific differences are very early apparent (*op cit*, pp 157-60).

Then the "Theory of Descent" is discussed, followed by criticisms of "Darwinism" and "Lamarckism" He admits "a great deal of probability" for the idea of "transformism" (of species and so on), especially as the arguments from geographical and geological distribution are not biological only. But he considers that the theory of descent tells us but little, as we have little or no real knowledge of the dynamic factors which cause it He admits rather grudgingly that it *may* be possible to ascertain the phylogenetic history of some smaller groups (p. 163), which certainly underestimates the work done on the history of the Horse, Camel, and Elephant,

to mention a few mammals only (*cf* Broom, p 64) But he is on more sure ground in criticising the various conflicting theories of the origin of vertebrates put forward by supporters of monophyletic development (*ibid*, p. 164).

Finally, he sums up the four known methods of deviations from inheritance. Johannsen's experiments show that continuous variations are not inheritable, provided that no mutants arise in the pure line The inheritance of acquired characters he regards as unproved Mendelism, interesting and important in its way, proves nothing about phylogeny as it is based upon variations already existent, and only tells us how they are handed on, not how they arise *ab initio*. Fourthly, he mentions mutations, but the treatment is very slight and does not mention the work of T. H Morgan He is right, however, in stressing the lack of plan or law in the origin of mutations under natural conditions.

Darwinism. He pays tribute to Darwin as a non-dogmatic scientific searcher after truth, but regards the "Darwinism" of his followers as mere dogmatism. Like most biologists he admits natural selection to be a *cause vera* (*ibid*, p 170), but points out that it is a negative factor. It causes the elimination of the unfit but does not account for the origin of anything Material for natural selection to act upon can only arise from mutations He does not allude to chromosome changes as a cause for the origin of new Linnaean species.

He considers that Darwinism fails to account for mutual adaptations, such as those between insects and plants, for the origin of parts whose utility is apparently non-existent, and for the origin of organs, such as the eye, which arise from different parts, and especially that it does not account for the early stages of organs which are useless though the mature organs are useful. Finally he points out that Darwinism does not explain restitutions (*op. cit.*, pp. 172-4).

This last argument is a clever one and indicates that Darwinism is absurd, at least as an explanation for these facts But a possible reply for Darwinians is that simple organisms possess powers of regeneration, but that, like other attributes of such creatures, they have been lost in more

complex animals Such a reply defends Darwinism from the charge of absurdity, and also supports the vitalistic position and Darwin's own belief in the special creation of life by showing that elementary organisms possess powers quite unlike those of inorganic bodies

Driesch does not deal with neo-Darwinism, as expounded, for instance, in Haldane's *Causes of Evolution* or Fisher's *Genetical Theory of Natural Selection* Yet, though these particular books appeared after Driesch's revision, their argument was known in broad outlines before 1928.

Lamarckism Modern Lamarckism, which, Driesch points out, differs materially from Lamarck's teaching, is criticised here The idea that organisms possess a faculty of responding to *any* change in environment is vitalistic; Lamarckism as expounded by Pauly (*Darwinismus und Lamarckismus*, 1905) has also a psychological or "psychistic" trend (*ibid*, pp 177, 179) Driesch, however, shares with most biologists the belief that the inheritance of acquired characters has not been clearly proved; though he sees a way out of this difficulty if Pauly's idea of adaptive characters as contingent mutations is adopted But even this will not save Lamarckism as G Wolff's criticisms of Darwinism apply to Lamarckism too (*ibid*, p 180) The phenomena of restitution remain inexplicable, as Driesch constantly argues; and he agrees with the Darwinians that Lamarckism breaks down before the facts of organisation among termites, ants, and bees, many individuals of which vary greatly but do not reproduce themselves

A short section on "Phylogenetic problems" (*ibid*, pp. 182-4) dismisses very cursorily the theories of Eimer and others about some law or laws of phylogeny, as indicating only the existence of an unknown principle, if evolution occurs at all; and neither here nor anywhere is Berg's *Nomogenesis* alluded to. The section closes with a list of questions to which Driesch sees no answers.

The whole of this "Part II" is in the nature of a parenthesis, except for his rejection of Darwinism as the only theory of evolution which makes a materialistic philosophy of the organism possible.

In "A, Part III; Organic Movements" (pp. 185-239), the positive argument for the autonomy of life is resumed.

Though the ordinary study of physiology is of little help, the "wholeness" of acts of movement demands consideration from the beginning. As an illustration Driesch considers three dogs crossing a road. The first, a normal dog, has to accelerate and run in a curve to avoid a carriage; the second has lost part of a cerebral hemisphere, so his movements are somewhat defective; the third can use three legs only. These instances correspond to variations in totality of external stimuli, in the intermediate organ, and in the effectuating or motor organ, respectively Yet the dog can reach his destination in each case. This illustrates the three kinds of regulation possible as regards actions (*ibid*, pp. 186-8). Woodger (*Biological Principles*, p. 449) criticises Driesch's philosophic assumptions here; but the criticism does not seem to me to affect Driesch's argument.

In lower forms of animal movement (Driesch, pp. 189-91) we find Individuality of Correspondence between stimulus and reaction, based upon experiments by Jennings and others. This contradicts J Loeb's earlier view that movements are merely reflexes and tropisms. The term "Individuality of Reaction" would, however, be preferable, I consider

Instinct is discussed in the second chapter. Driesch defines it, rather queerly, as one of the higher classes of animal movements. But his discussion, though interesting, need not be dealt with in detail, for he finds in instinct only some indications of vital autonomy, "no real absolute proofs" (*ibid*, pp. 192-201).

Such a proof is found under the head of Action ("Chapter 3", pp 202-35). He takes acting organisms, including men, as merely "natural bodies in motion", thus avoiding "pseudo-psychology", and agrees with the work on behaviourism done by Watson and others, so long as it is not taken to imply an *a priori* mechanistic conception of life. This attitude is fair and reasonable.

His definition of action (*op. cit*, p. 203) is important. He will not take it as equivalent to movement of any sort, only to movements which depend not merely on an actual

stimulus but on the specificity of all past stimuli received by the organism and on their effect For this the term "experience" may be used Thus, when Jennings found that *Stentor* reacted in various ways to a stimulus, finally swimming away, no experience was demonstrated But it was shown when, on the experiment being repeated, *Stentor* swam away at once Such actions are clearly exhibited in all vertebrate classes and in many invertebrate classes (*op. cit.*, pp. 205, 235-9)

But acting is best examined in Man, considered as a material system in motion, all psychology being excluded (*op. cit*, p 207).

As a first criterion acting or reacting has an historical basis, but it is a general one, not specific, still less mechanical like a gramophone record which gives the same tune every time it is played. The effects of random movements *may* form part of it, as is noticed in babies Effects gained by "trying" are used for future problems, and what psychologists term association also forms part of the historical basis of acting. Pavlov's experiments are referred to here (*op cit.*, pp 206, 238) Driesch finds himself obliged to use psychological terms occasionally but he avoids speaking of "memory"

Even in the historical basis of action alone he finds a disproof of mechanism and a proof of vitalism Learning by trial means that a material system M, when affected by a stimulus S, produces various reactions $R_1$, $R_2$, $R_2$ . $R_n$ The important point is that if M is affected by S a second time it reacts at once to $R_n$ Learning by association can be summarised as M affected by S, with which another event T is associated, though T does not itself evoke a reaction But later T alone will evoke the reaction due originally to S (*op cit.*, p 212).

But, as he rightly says, there is nothing like this in the inorganic world (*op cit.*, p 213). To give one crude illustration of my own, a compass needle will turn when a magnet is brought near and simultaneously a book is opened. But opening a book will not deflect the needle however many times the compass has been previously subjected to the double experience (The incongruity of the term "experience" when applied to an inorganic object is illuminating;

the word is nonsensical except for organisms considered as essentially different from the inorganic If Pavlov could have shown that a compass needle could associate the sound of a bell with the effect of a magnet he would indeed have gone far to destroy the vitalistic differentiation of the living from the non-living )

To sum up, while some movements are reflex, in others there is evidence of capacity for learning by trial and error or by association, and this acquisition of experience is a proof of vitalism.

"Individuality of Correspondence" is the second criterion of action (*op. cit*, pp 213-16).

Stimuli may be simple or "individualised"; the latter means a specific combination of single elements. Such a stimulus always evokes action—not a mere reflex movement. The reaction to a specific person, melody, or such like, is different from the movement that may be evoked by seeing an unknown person or hearing an unknown tune The reaction is not constant; it is individualised, but quite differently from the individualisation of the stimulus which evokes it. Here is another difference between the Organic and Inorganic

This individuality of correspondence between stimulus and effect is illustrated with an example used by Bergson, but dealt with by Driesch from a behaviouristic point of view. Two friends meet, and A says to B, "My brother is ill." B will react by certain sounds, possibly by action. If "mother" is said instead of "brother" the mechanical difference, of sound, is very slight; but the effects of the two stimuli will be very different. Again, "My brother is ill" might be said in any language known to B; the mechanical difference is great but the stimulus is unaltered.

Driesch justifies his use of a sentence as a stimulus in a footnote (p 215); the general mental condition of B, his "historical basis," certainly affects his reaction but it does not cause it.

Now the stimulus is not an aggregate or resultant but a totality, and its totality or meaning may be the same even if all its details are altered (as when a different language is

used); the reaction remains the same. Yet a small change in the stimulus may produce a great change in the reaction. But the reaction is a totality too (*ibid*, pp 216-17)

So Driesch considers that life-autonomy is shown by the second criterion of acting as by the first But the proof from the second criterion does not seem to me so sound as that derived from the historical basis of acting. For instance, it may be objected that the reaction to "My brother is ill" is affected largely by the single elements "My", "brother", "ill", "is." The reaction differs if it is somebody else's brother, if it is some other relation, if the predicate is different, if the tense is past or future instead of present

But that human action is a valid proof for autonomy of life, at least human life, seems certain And Busse and Bergson, in *Matter and Memory* (1896) (*cf* III(*b*), 2, iii, below), have both adopted the same position, though with different methods.

This third proof of vitalism is quite independent of Driesch's first two proofs, based on (i) harmonious and (ii) complex equipotential systems in morphogenesis; and the two criteria of action are complementary and the argument for vitalism can be formulated from their union (*op cit.*, pp 220-21).

Driesch prefers to term the autonomous principle discovered in action the "psychoid"; but most people use entelechy, as Driesch does for both his earlier autonomous principles. He avoids psychological terms such as mind, soul, psyche, as his treatment is non-psychological throughout, as far as possible (*op. cit*, p. 221).

From a study of "cerebral physiology" Driesch argues that the material brain is not the basis for action; "memory" and "understanding" are not functions or secretions of the brain, though it may be *related* to them "in an unknown way" (*op cit.*, pp. 225-30) This part is very compressed, though numerous experiments are referred to. Bergson's treatment is much fuller.

Similarly the "Regulability of Movement with Regard to the Motor Organs" (*ibid*, pp. 230-35) is too compressed for further condensation, and the vitalistic conclusions drawn

from experiments, though interesting, are subsidiary to the main argument discussed above

So ends "Section A" on "the Science of the Organism."

The second volume, "Section B", deals with "The Philosophy of the Organism"

In its introduction (*op cit*, pp 243-4) teleology is referred to for the first time It was avoided previously because of its personal, psychological content. A man's *willing* is purposeful, but *science* should not use this term. However, as the corresponding scientific word has not yet been invented, Driesch is now prepared to use "teleology" *analogically* in connection with organic phenomena. A man-made machine is in itself purely mechanical or physico-chemical, but it shows teleology as a man means it for a definite purpose. Many mechanisms in nature may be said to show adaptation to an end and so can be called teleological in the popular sense. Driesch calls this static teleology of constellation. Where entelechy is proved to act he speaks of dynamic teleology—by analogy again; but the psychoid shows this in a more advanced form than the entelechy associated with morphogenesis

Entelechy always produces manifoldness which is extensive in space, but it is not in space itself it is "manifold in thought but simple as a natural agent" (*ibid*, p. 245). Provisionally Driesch speaks of different ranks or orders of entelechies (*ibid*, p. 246). But this has an ill-omened sound, redolent of long discarded *blas* and *archaei*, and accords badly with the simple fundamental conception of entelechy associated with each of his three proofs of vitalism In other parts of the book he speaks of entelechy in the singular; only at the end (*op. cit*, p. 329) does he state clearly that "there is one entelechy" and the "sub-entelechies' previously spoken of must "in any case" be thought of as connected.

"B, Part I" discusses "Entelechy and Physics." Here and elsewhere, apart from the philosophical question of free will, he ignores all recent work on indeterminacy in atomic physics (*vide* my III(*c*), 1). Vitalism, as he suggests, needs not be equated with indeterminacy; but the apologist for vitalism need not press determinism beyond its present

limits, or apparent limits (*cf. op. cit*, pp 248-9; and Woodger, *Biological Principles*, p 427).

His discussion of the relation between Entelechy and Causality (*op cit*, *B Part I, B*, pp. 250-85) is abstruse, and his treatment of conservation of energy and the second law of thermodynamics can be omitted in a work on vitalism. What needs attention is the manner in which Driesch relates entelechy to them

He accepts the principle of the conservation of energy for organisms, including the body of Man, and Ostwald's idea of an unknown form of energy, active in morphogenesis and, probably, in "nervous phenomena", even though small in amount, is rejected, though attractive to the vitalistic thinker. The possible existence of such energies would not affect entelechy or vitalism according to Driesch All forms of energy, he affirms, are quantitative, or capable of measurement in terms of work But entelechy lacks all characteristics of quantity, it is order of relation, so it is not an energy and does not disturb the law of conservation of energy (*ibid*, p 257)

Driesch's attempted solution of the problem of entelechy and entropy is well known He considers that entelechy can suspend any one of all reactions that are possible in a given case and which would happen without entelechy. Also it can allow to occur any reaction which it has suspended, though it cannot cause a new reaction to take place. It is thus an agent but not a physico-chemical one

Hence he deduces that entelechy must have always acted, that is, that life is continuous. This points to a beginning of life and entelechy, as physical reasons show that life cannot have existed externally on the earth (*cf* my Part II(*c*)) But, unlike Darwin and Wallace, Driesch will not admit the only solution to this problem—that of a creation of life or "primordial act of suspension of inorganic becoming" (*op. cit*, p. 263). He merely states "we know nothing"; which is reasonable for science but very cautious for a philosophy of the organism. Later, however, he rejects abiogenesis entirely—even as a source of life in the dim past (*op. cit.*, p. 302).

He suggests that entelechy manifests itself in the formation of enzymes which, once they are formed, work on purely chemical lines (*ibid*, p 263) It is amazing that this great topic should be so scantily treated by Driesch The occurrence just at the right time and place of enzymes is an argument for vitalism which grows stronger every day as new enzymes or zymogens are discovered (*cf* my III(*b*), 3, iv).

Entelechy and Mechanics are discussed in "Chapter 3" (pp. 269-78), with much repetition of previous work Driesch does not consider the modern physics made familiar by Jeans, Eddington and other writers, and takes mechanics in the Newtonian form for simplicity; but he claims that his arguments may be easily transformed to suit other systems (*op. cit*, p 270) He states that the suspension of energetic processes and the potential relaxation of this suspension by entelechy do not contradict the laws of physics; and certainly the energy in the physical system can be unaltered, as, for instance, if a pendulum is held up at the point where all its energy is, momentarily, potential But how can a pendulum be checked except by a force or resistance from outside the pendulum? Yet entelechy is "non-energetical" (*op. cit.*, p. 56).

Driesch's treatment of such problems can only be regarded as incomplete, not to say weak He says, finally, "The laws of pure mechanics are broken in *any* case", and it seems best to leave it at that Life is autonomous and therefore living things do not entirely obey the laws of mechanics (*cf. op. cit.*, p 277). The same difficulty arises when the reaction of mind and matter is considered by other writers

He is on sound ground when he points out that entelechy affects only general features of organisation, not the details of individual molecules; though the possibility that, as Eddington suggests, *brain* particles *might* be affected by mind must not be lost sight of (*cf.* my III(*b*), 2, iv).

"Chapter 4, How Entelechy is affected," calls for little comment: much of it has been dealt with in connection with earlier chapters. The summary is that entelechy is called into activity by changes of normality as a whole in any system it governs, or, rather, is inherent in. "It only interferes at certain points and at certain moments and then leaves

matter to itself." Quantitative changes in entelechy are impossible as entelechy is essentially non-quantitative The question whether entelechy is a special type of causality is postponed (*op. cit*, pp 282-5)

"Part I, C" discusses "Entelechy and Substance." But it gives us little further information about entelechy, although many aspects of biology are touched on in a stimulating way Driesch rejects the idea of a "living machine" but does not do justice to the invariable connection of life with the mixture of colloids termed protoplasm

In philosophical language it is possible that entelechy may be one "substance", matter being another, and both forming part of Nature (*ibid.*, p. 297). But we do not know (*ibid*, p. 300). Entelechy cannot be divided, for divisibility implies spatial dimensions, which entelechy does not possess. So it cannot have a seat like Descartes' pineal seat of the soul; though it may have points of mutual relation with an organism (*ibid*, p 299).

The gist of this "Part" is the admission that, "so far", the characteristics of entelechy are little more than a series of negations (*ibid.*, p. 300). This is the weak point in Driesch's exposition of his theory, and critics have not been slow to seize upon it (*e g.* Wightman, *Science and Monism*, p 352), as though this were the end of the book Actually, though very unforcibly, Driesch puts forward a better, positive, view of entelechy under the incongruous heading "Biology and Logic" in his "B, Part III", a section easily overlooked by critics who are not interested in Driesch's views on logic and metaphysics. This is a serious example of the shortcomings alluded to below (p 159).

Here is the necessary supplement to the dreary negations of his page 300, and it is on the lines I have indicated below as necessary. Entelechy is the "very nucleus" or "essence" of a living person; it is the ultimate source of the restoration of "wholeness" after disturbance in organisms; and so on (*ibid.*, pp. 312-13). True, his criteria of what "wholeness" means are entirely unsatisfying (*ibid*, pp. 315-16); but it is enough to confess, as Driesch does on page 314, that proper definition is impossible, because wholeness is a fundamental concept, inexplicable in terms of simpler ideas.

Driesch then considers what forms of causality are possible if not actual (*ibid*, pp 318-22), the treatment being exactly the same as in his earlier *History and Theory of Vitalism, Part II, D.*

There are, theoretically, four possible forms of "becoming" or of "causality" (*a*) the parts of a given system may change independently of one another; every single change or event may be related to a previous single change or event; the manifoldness is the same if the changes occur within the system or is of a simple order of increase if affected by simple change from outside. Here we have *singular* (or additive) *causality.*

(*b*) A system at rest, though unaffected by causes from space outside, might begin to show change. Here the postulate of causality may be abandoned; or some creative causality postulated as working into space; that is, *motion creating causality.*

(*c*) The number of material elements in a system may be suddenly increased although none enters from outside the system. Again the concept of causality may be abandoned; or a *matter creating causality postulated*

(*d*) A system may change from a state A at a time $t_1$ to a state B at $t_2$ in such as way that, though no becoming has been initiated in a system at rest (*b*) nor has the number of atoms or elements been increased (*c*), the manifoldness of spatial relations among the elements of the system has been "greatly" increased, though nothing within the system or outside it in space accounts for the change (*a*). As an illustration a chaotic assemblage of atoms or elements may become grouped into an arrangement indicating wholeness, say the form of a fish or flower. Again the concept of causality may be abandoned; if not, a creative causality need not be hypothecated here as (*b*) and (*c*) cover the creation ideas; but there might be a new type of causality which should be called *individualising* (or unifying) *causality.*

I think it must be conceded that all possible temporal sequences, leading to the data of empirical science of the "Now-here-atom" type, must fall within Driesch's four forms of causality.

Which of these possible forms are realised? (*b*) and (*c*), it

will be generally admitted, are not empirically realised "as far as we know"; to which many would add, "except most probably at a certain time or times in the remote past." All will probably agree that the first form covers the realm of inorganic nature.

Is the fourth form actually realised? Mechanists would deny it; vitalists agree with Driesch that it is realised in the realm of living things, whether or no they limit their ideas of the autonomy of life to those claimed by Driesch for entelechy The point here is that a logical justification is provided for the theory of life autonomy (*op. cit*, p 322).

"B, Part II, Biology and Psychology," is brief, and must be dealt with briefly, as a volume could be written about entelechy in relation to psychology Driesch rejects psycho-physical parallelism by a treatment similar to Bergson's; the brain is not an independent mechanism but is used by entelechy, as Driesch says, or by mind, as Bergson and others would say His discussion of psycho-physical interaction is, however, full of ambiguities

"The Living of Life", that is of human life, is illustrated by a single example. I "will" to drink water; that is all that occurs consciously till I become conscious that my wish is fulfilled Meanwhile various actions occur: first and most mysterious of all, a brain motor centre is stirred into action; then a motor nerve is stimulated, certain muscles of arm and fingers contract to grasp a glass, "etc., etc." But these processes are not consciously directed; most human beings are quite ignorant of some of them (*ibid*, p 307).

Critics might say that people do not *will* to drink water. When the body is short of water a stimulus affects the sensory brain matter and then reflex action takes place And easy access to drinking water is assumed But Driesch's argument holds if a book is substituted for a glass of water; there is no bodily demand for reading, though the mind of a thoughtful person often requires its corporeal frame to lift, open and hold books till the mental thirst for reading is satisfied.

As such bodily processes are initiated by a conscious impulse I cannot see how the conscious part of the whole episode can possibly be regarded as an epiphenomenon even to the entelechial states of the body, as Driesch suggests

(*ibid.*, pp 307-8). The entelechial and bodily events are post-phenomena to an original volitional act, at least in my development of his illustration.

Driesch dismisses the connections between bodily reactions and such mental experiences as volition, emotions, suggestion, auto- and otherwise, with the remark that almost nothing is understood here He is disposed to agree with Bergson and Carr that the often alleged connection of a specific brain state with a specific conscious experience is erroneous Finally, he toys with solipsism (*ibid.*, p 309); but soon afterwards he declares stoutly that "in practice" there are many persons in the world and not one only (*ibid.*, p 312)

"B, Part IV, Biology and Metaphysics" touches on various philosophical questions which need not be considered here, not even his stimulating discussion on "the One and the Many."

But a few concluding criticisms may be made upon the book as a whole.

The treatment is over-elaborate, following too closely the historical and logical development of Driesch's own thought. Thus, his main summary concerning entelechy is a mass of negations (*ibid.*, p 300), and only later comes a too brief positive exposition (*ibid.*, pp 312-13; *cf* p 156 above). The suggestion of different entelechies working in the same organism on page 246 is only corrected on his page 329. Frequent repetitions occur, and they are burdened with unduly lengthy references to later parts where a mere "*cf* page . . ." would have sufficed Questions are frequently raised, but the answers, or even admissions of ignorance, as for "wholeness", are postponed again and again. Thus the book is too long and unnecessarily difficult to follow.

This last is a general complaint, as Rádl states (*History of Biological Theories*, Ch. XXXII, "Driesch's Logic"). He suggests that this has hindered scientists from adequate examination of Driesch's principles, and thinks that the obscurity is in Driesch's thought, not his style. This, I consider, is partly correct, though Rádl's philosophic analogies are not to me very convincing. Nordenskiold too complains

that in "his profound and far-reaching analysis" Driesch enters "a maze of abstract speculations" and uses "extremely complicated terminology" (*History of Biology*, p 609)

Although the Gifford Lectures were rewritten in 1928 the references to experiments have not been increased, and this suggests that the experimental basis is not up to date. But the preface explains that reference is only required for the early experiments which made discoveries

The cautious attitude evident throughout is reasonable and commendable up to a point, belief in a special power in living beings is only obtained by slowly demonstrating that inorganic forces do not account for the phenomena observed. But the argument would have been strengthened if Driesch had collected his various *indicia* for vitalism as an appendix to his three proofs (*cf* my III(*d*), 2, iii).

To the suggestions, made by Bergson (*Creative Evolution*, p. 44, footnote), and Wightman (*op cit*, p. 352), that entelechy should be rejected, the reply, I think, should be as follows

Firstly, this concept of a "regulatory" entity, directing the course of energy without consuming it, has done good service in helping to destroy old erroneous ideas of vital force as a form of physical energy, while not yielding to the absurdities of mechanism as a complete account of the growth and life or organisms. Secondly, where Driesch is positive he is right. Human action and the facts of the two equipotential systems are valid arguments for the existence of a controlling power present in living organisms but absent in dead ones and non-living matter. Driesch weakens his case by not putting forward the other arguments for vitalism and by his agnostic attitude concerning the origin of life, although he belatedly admits sufficient knowledge to repudiate the mechanistic hope in primitive abiogenesis (*ibid.*, p. 302). But, thirdly, his experiments and arguments have revived belief in a better conception of what the old term "vitalism" stood for. So in connection with those life manifestations which Driesch expounds so ably entelechy remains a useful term. Yet it is not an adequate expression of life-agency in the limited and partly negative form in which he employs it.

What is needed is a concept of life as an agent—in a

category of its own, positive, parallel with rather than opposed to inorganic forces, but not to be decried and mystified as not being exactly as they are. Analogous to this life-agent category are the differing categories—or is it one category really?—of philosophy and psychology; human will, affections, and so forth act in a universe of electrons, energies, and the like, but defy expression in mechanical formulae. So, too, the category of religious experience, as Whetham, J Needham, and others testify (*cf.* my III(*d*), 2, iv). Only life, or whatever term is chosen, can account for the phenomena which are peculiar to organisms in general, animals even more so, and Man most of all (*cf* my III(*d*), 2).

(iii) *Driesch and modern vitalism*

In many authoritative modern works on biology and the history of science there is little or no reference to the work of Driesch. He is not mentioned in the Histories of Locy (biology), or Sedgwick and Tyler (general science) This applies, too, to the articles in the *Encyclopaedia Britannica* (1930) on *Life*, *Biology*, and the *History of Biology* by the late Sir J. A Thomson and Dr. Singer, who has only one reference in his *History of Biology*. Rádl (*History of Biological Theories*, p 363) argues that Driesch left biology for philosophy "before the war", and Nordenskiold (*History of Biology*, p. 609), says the same.

In Part II(*d*) I have shown that belief in the autonomy of living things never died out but was maintained by excellent biologists even during the zenith of mechanism. Also, Driesch failed to emphasise numerous evidences for vitalism, some of which are more apparent to an outdoor naturalist than to a laboratory worker (*cf.* III(*d*), 1, below).[1] So I am unable to accept the view that Driesch is the sole author of modern vitalistic thought.

But Rádl praises Driesch's work for vitalism (*op. cit.*, Ch. XXXII), though his estimate that Driesch largely destroyed Darwinism (*ibid.*, p. 352) is curiously discounted by his later statement that Driesch really "succumbed to

[1] Professor Driesch has informed me that he analysed these in *Die Maschine und Der Organismus* (Leipzig, 1935, pp 10-29), but this paper has not been available in England.

Darwinian ideas" (*ibid*, p. 370); though this is at least a great exaggeration Anyhow, the great facts remain that modern vitalism can be conveniently dated from Driesch's experiments on sea-urchin eggs in 1895, and that in his monumental Gifford Lectures he has given a logical disproof of the machine theory, based upon a mass of experimental work in embryology and upon behaviouristic treatment of human action which agrees with the findings of Bergson, McDougall, and other modern psychologists

The influence of Driesch upon current biological thought is notable, more outstanding than that of any other single biologist, except perhaps T H Morgan For instance, even J Loeb (*op cit*, *passim*) and J Needham (*op cit*), who are both strongly anti-vitalist, refer continually to Driesch's classical experiments, as does E W MacBride, whose outlook is very different (*e g Introduction to Heredity*, 1931, p 63 *seq*). Johnstone's *Philosophy of Biology* is based upon his work and that of Bergson (but *cf* III(*d*), 1, i) Woodger says that biologists should be grateful to Driesch for dealing with aspects of the organism omitted by other writers (*Biological Principles*, pp 266-7) Merz (*European Thought*, ii, 455, 549) and Broom (*Coming of Man*, p 224) write with appreciation; Broad, however, finds his arguments not in the least conclusive (*Mind and its Place in Nature*, p 58) Such authors refer, very properly, to Driesch by name. But his influence is also strongly apparent even where no acknowledgement is made, for instance, in Dr. J. Gray's notable address to the British Association in 1933 (*cf* III(*d*), 1, iii), and in Durken's *Experimental Analysis of Development* (III(*b*), 3, v) Such examples could easily be multiplied.

The debt which biology in general and vitalism in particular owe to Driesch can be partly summarised in the words of a largely hostile modern critic. Dr. W. P. Wightman, although he rejects entelechy, possibly because of incomplete appreciation of Driesch's confused presentation of its real significance (*cf* p. 159 above), and although for *a priori* reasons his own preference is for a philosophic monism, to which vitalism is fundamentally repugnant, admits that Driesch's experimental results in morphogenesis "are such that to explain them the mechanist is driven to

make assertions which, in my view, reveal the essential 'silliness' of his theory" (*Science and Monism*, pp 345-7, 350)

And the same may be said of his argument from human action, though in this a personal, experimental contribution is not possible as for his two first proofs.

## III(*b*).—PHILOSOPHY, PSYCHOLOGY, AND MODERN BIOLOGICAL THEORY

"Mind orders all things"
ANAXAGORAS, quoted by Fabre
(*The Glow-Worm*), 1919, p 234)

CHAPTER I

# PHILOSOPHY

### (i) *The general trend of modern philosophy, the New Realism*

VAST and often mutually destructive tomes have been and are being written on philosophy; weighty and discrepant books appear upon its history (*cf* B. Russell, *Our Knowledge of the External World*, p 13) A single chapter cannot summarise the first group or try to compete in the second. All that is attempted here is an appreciation of the effect of recent philosophy upon the vitalistic outlook in biology

Whetham, in his valuable summaries of the reactions of science and philosophy in Chapters VII and X of his *History of Science*, has pointed out that, "during the greater part of the nineteenth century, most men of science, especially biologists, accepted uncritically the model of nature put together by science as ultimate reality"; though "some of the physicists and philosophers were more cautious" (*op cit.* p. 316) This emphasis upon the credulity of biologists is admirable. Evolution, only a scientific theory for Darwin's "modest mind", itself became a philosophy, to some almost a creed (*cf.* II(*b*), iv and v); and, though linked with materialist determinism, this "evolutionism" was for a time an optimist philosophy (*op. cit*, pp. 445-6). For, "with each great advance in science . . . the human mind, by an inevitable exaggeration . . tends to think that it is on the point of reaching a complete mechanical explanation of the Universe."

So, "at the beginning of the twentieth century," as Whetham

continues, "the majority of men of science held unconsciously a naive materialism, or . . . inclined to the phenomenalism of Mach and Karl Pearson, or the evolutionary monism of Haeckel or of W. K. Clifford" (*loc. cit*, and p 339)

But philosophy has lately become more critical of, even hostile to, scientific naturalism

William James advocated pragmatism: "The sole test of truth in a belief is whether it is useful." This I do not accept, except to a very limited degree. But, for what it is worth, it supports vitalism, for it is obviously often "useful" to regard men and animals as animals, not merely as peculiar physico-chemical systems But, as Whetham remarks, some beliefs at least can be put to the tests of observation and experiment, although this is not strict pragmatism (*ibid.*, p. 447)

James Ward (*Naturalism and Agnosticism*, 1899), and W. R. Sorley (*Ethics of Naturalism*, 1885, 1904), criticised materialist ethics, concluding that "an idealist interpretation of the Universe is as necessary for secure ethics as for rational metaphysics" (Whetham, p. 339) The thought of Bergson is dealt with in the succeeding chapter on psychology Here he is to be noted as a leader in the modern philosophy which opposes mechanism and, in Bergson at least, supports vitalism in biology Hobhouse and other philosophers at the end of the nineteenth century were stating the case for philosophy against the uncritical assumptions of the scientists, and gradually the scientists, or some at least, began to take notice.

The New Realism abandons philosophic systems which start, like scholasticism, from some *a priori* theory, also idealism, which makes reality depend upon our thoughts, also phenomenalism; for it proceeds "piecemeal", in the scientific way, and recognises the persistence "in some way" of the entities or "objects" with which sensations and mental concepts are allied (Whetham, p. 448). This seems to me to be what Bishop Barnes calls "moderate realism." In this philosophy, which I accept, the results of mathematics and science are employed as a basis and so philosophy becomes linked with other knowledge (*cf.* Whetham, p. 449).

An important part of this system is its treatment of logic. Whetham points out that some philosophers were misled by

the traditional deductive system of logic; but "luckily, informal methods of reasoning grew up among practical men of science" Whewell emphasised the fact that the success of deduction depends upon its starting from the right idea; absence of this was the weak point in much syllogistic argument of the formal type J M Keynes pointed out that an induction may approach certainty, but that it is a fallacy to regard each, or even any, induction as an infallible rule which cannot ever have an exception (*Treatise on Probability*, 1921; *cf.* Whetham, pp 449-60) So the "laws of nature", exaggerated by French mechanistic philosophers in the eighteenth century and unduly deprecated by Mach in the nineteenth, are nowadays taken in a medium way. And the term "law" is misleading in this connection as its ordinary meaning implies a "kind of moral obligation"

"Laws" established by induction are only probabilities; the probability may be enormous, "but the infinite probability of certainty is never reached." This result of the critical examination of the process of induction from Hume to Keynes is confirmed by certain kinds of experience, as Whetham points out (*op cit*, p 460). Thus one of the foundations for philosophical determinism, belief in the universal validity of "natural laws", is shown to be much less secure than it appeared to be to the materialists of the nineteenth century.

It is important to distinguish this philosophic attack upon rigid determinism from the "indeterminacy" deduced by our leading physicist philosophers directly from modern physics, dealt with in Part III(*c*), 1. The result in each case is to weaken the basis of materialism and so of mechanistic views in biology.

(ii) *A. N Whitehead; Holism, Monism, etc*

I find, like Dr. W R Inge (*God and the Astronomers*, p. 116), and Dr. Wightman (*Science and Monism*, p. 399), that Professor Whitehead is difficult to follow, for, more even than most philosophers, he uses ordinary terms with peculiar meanings. Inge also speaks with similar frankness of C. D. Broad; and I would include Bertrand Russell sometimes in the same category. But certain ideas of Whitehead's are of importance here.

After speaking of the seventeenth-century mathematicians, he states that the enormous success of their scientific abstractions of matter and mind "foisted on to philosophy the task of accepting them as the most concrete rendering of fact. Thereby, modern philosophy has been ruined. It has oscillated between three extremes There are the dualists, who accept matter and mind as on equal basis, and the two varieties of monists, those who put mind inside matter, and those who put matter inside mind. But this juggling with abstractions can never overcome the inherent confusion introduced by the ascription of *misplaced concreteness* to the scientific scheme of the seventeenth century." This "Fallacy of Misplaced Concreteness" is the error of "mistaking the abstract for the concrete" (*Science and the Modern World*, pp 70 and 64-5) )

In between the mind and material of this duality, he continues, "there lie the concepts of life, organism, function, instantaneous reality, interaction, order of nature, which collectively form the Achilles heel of the whole system" (*ibid*, p 71; *cf* E S. Russell, III(*d*), 1, below).

Then, after dealing with the failure of the idealist philosophies to make connection with "the facts of nature", Whitehead states, "A further stage of provisional realism is required in which the scientific scheme is recast, and founded upon the ultimate concept of organism" (*ibid.*, p 80).

"... the whole concept of materialism only applies to very abstract entities, the products of logical discernment. The concrete enduring entities are organisms, so that the plan of the *whole* influences the very characters of the various subordinate organisms which enter into it ... the mental states enter into the plan of the total organism (in an animal) ... an electron within a living body is different from an electron outside it ..." and so on. "But the principle of modification ... represents no property peculiar to living bodies" (*ibid.*, pp. 98-9).

So Whitehead opposes materialism. But, as the last-quoted sentence shows, he is not a vitalist at all Vitalism, he says, "is really a compromise. It allows a free rein to mechanism throughout the whole of inanimate nature, and holds that the mechanism is partially mitigated within living bodies. I

feel that this theory is an unsatisfactory compromise The gap between living and dead matter is too vague and problematical to bear the weight of such an arbitrary assumption, which involves an essential dualism somewhere" (*ibid*, p 98)

I personally do not care if dualism or polyism does crop up in dealing with differences between parts of nature As Bishop Barnes says, "We have no reason to assume that . . all the processes of nature will be even theoretically united into a single coherent theory" (*op cit*, pp 552 *seq*) Berg agrees with this (*Nomogenesis*, p 20) There is, I maintain, in the thought of many philosophers, a "Fallacy of Misplaced Simplicity", which leads them to ascribe to Nature or the cosmos a monism for which there is, at present at least, no foundation in fact B Russell and Wightman as well as Whitehead and others cherish this fond hope. The only likely approach to such a monism is that advocated by Sir James Jeans, among others, in which matter is a manifestation of mind; but this does not fuse matter and mind into one (*The Mysterious Universe*, pp 148-9) But not even the most devoted theoretical monist jibs at reference to protons and electrons, which is dualism, nor to the recent additions of neutrons and positive electrons, which is practical "polyism", or what Broad would call "pluralism" Jeans points out another duality in electrons and protons, "both exhibit a dual nature" as particles and wavicles (*ibid*, pp 39, 43) So I cannot appreciate why such a sturdy thinker as Professor Whitehead should regard "dualism somewhere" as an objection to admitting the reality of the difference between a live organism and non-living nature Broad illustrates the difficulties that crop up if a fetish is made of monism or any other numerical -ism in the first chapter of his great book on Mind.

"Science," Whitehead continues, "is taking on a new aspect . . . neither purely physical nor purely biological. . . . Biology is the study of the larger organisms; physics is the study of the smaller organisms" (*ibid.*, p 129). But the macrocosm which really corresponds to, say, a molecule is not a Man, or even a beast, but a star or a galaxy. So vitalistic biologists can only admit Whitehead's idea of organism to a very limited degree. They see analogy, not

homology, between the unity of a molecule and that of a tree or a tiger They prefer to use "organism" in its ordinary, restricted, scientific meaning, and such a term as "whole" for the unities which Smuts, Whitehead, and others have emphasised in all sorts of natural entities from atoms up to galaxies and Man.

Biologists can, however, agree with Whitehead about "the event as the ultimate unit of natural occurrence", and that "colours, sounds, scents, are required for nature and are not emergent from it" (*ibid*, p. 129). As Whetham says, "By the light of the new realism, which he himself has done much to formulate, Whitehead takes much the same view as Moore and Broad, and seems to restore to us a scientific theory of the world of beauty and moral values" (*History of Science*, p 479; *cf* my III(*d*), 2).

"Holism" is expounded by General J. C Smuts, F R.S., in *Holism and Evolution* and in various articles, and this theory is popularly associated with him. General Smuts himself is hostile to Driesch's entelechy (*op. cit*, pp. 171-2) and to any specific idea of a life-force; and though he mildly sympathises with a general vitalistic outlook (*ibid*, p. 160) he regards vitalism and mechanism as both very inadequate. But the idea of wholeness is also prominent in Whitehead's philosophy; Smuts and he published their books together in 1926; and, after Smuts presided at the British Association Centenary in 1931, it was favourably mentioned by many eminent scientists before that Association in 1933 and 1934 (*vide* III(*d*), 1, for discussion of wholeness in biology). So the concept of Wholes is undoubtedly significant in modern scientific thought; and, though I reject the assumption, explicit in Whitehead and partly so in Smuts, that physical and biological units are homologous, the central idea that a whole is not explicable in terms of its separate parts accords well with the vitalistic position summarised in my III(*d*), 2.

There remains neutral monism. The leading exponent of this theory, which holds that matter and thought are both forms of a single, assuredly very tenuous substance, is the present Lord Russell, who writes "Mind and matter are for certain purposes convenient terms, but are not ultimate

realities. Electrons and protons, like the soul, are logical fictions" (*What I Believe*, Kegan Paul, 1925, p. 17; quoted by Streeter, *Reality*, p 17).

On imperial and educational matters, in which I have not inconsiderable experience, I can only regard Bertrand Russell, as he often styles himself, as an unduly pessimistic and extremely unsound authority, and, for these reasons, I admit that I view with suspicion his pronouncements upon philosophical affairs Canon Streeter is certainly right in commenting on the passage just quoted that, anyhow, "The 'soul' stands for an element in Reality which can frame theories about electrons and protons."

But elsewhere B Russell admits that, even on the determinist theory, "Free-will . is true in the only form which is important" (*Our Knowledge of the External World*, p. 236). If, then, "mind and matter are both composed of something more primitive, which is neither mental nor material" (Whetham, p. 474), as neutral monism teaches, again we find a modern philosophical theory which definitely opposes mechanism except as a limited logical system based upon "fictions" Also, the vitalistic argument from human personality is allowed as relatively "true"

I do not employ these as vitalistic arguments myself, for I hold with McDougall (Ch 2, v, below) that the neutral monism idea is quite wrong, and so, therefore, are the notions founded upon it As Woodger says, "the desire for monism" always operates against vitalism; though "vitalism cannot be refuted merely by 'expressing your feelings'" (*Biological Principles*, p 205).

If monism seems indicated to any person from his researches in science and philosophy, he is, of course, right in proclaiming it; but too often "the desire for monism" seems to me to be a "last infirmity of (moderately) noble minds", which, abandoning other "idols", nevertheless worship an *a priori* assumption similar to Oken's spherical "All" (*cf.* Woodger, *op. cit.*, p. 458).

(iii) *Emergent Evolution and Vitalism; Teleology; J. S. Haldane.*

The term "emergent" was suggested by G. H. Lewes (*Problems of Life and Mind*, 1875) for anything of which the

nature cannot be foretold from knowledge of its constituents as they exist prior to the emergence. Emergence is now a fairly influential school of modern philosophy or ontology, closely allied but precedent to Holism. Its chief exponents are S. Alexander (*Space, Time and Deity*, 1920), C Lloyd Morgan (*Emergent Evolution*, 1923), and C. D. Broad (*Mind and its Place in Nature*, 1929) As Professor Wolf admirably sums up, like Bergson in his *Creative Evolution*, they emphasise the "originality of natural events and the utter impossibility of predicting the character of most results from a mere knowledge of the laws of matter and motion; especially so in the case of vital phenomena and the higher activities of human beings" (E.B., 15, 333b).

Lloyd Morgan's *Emergent Evolution* is well criticised by Woodger (*Biological Principles*, pp 105-10, 118 *seq* ); though Morgan admits the need for an adequate theory of knowledge, his beliefs in God, an external world, and other matters, are insecurely based on what he calls "acknowledgement." His scheme also involves many other peculiar ideas and phrases, and I share Woodger's uncertainty of comprehension of a theory which seems to assert mainly that evolution consists of a series of events that are not understood (*cf.* Woodger, p 110)

As applied to the problems of biology this theory is termed Emergent Vitalism, in contradistinction to the "substantival" vitalism of Driesch, as by Broad, whose reasons for rejecting Driesch's arguments appear to me quite inadequate (*op. cit.*, p. 58). But as the same principle is applied to the differences found in inorganic nature, such as those between electrons, atoms, and molecules, it lies outside the succession of vitalistic beliefs which have regarded living things as radically distinct from all other kinds of matter, and so receives but brief mention in this history. Emergent vitalism as outlined by Broad (*ibid.*, pp. 58, 67-9, etc.) is mere supposition, and seems to me to conflict hopelessly with such facts as biogenesis (II(*c*), iii) and entropy resistance (III(*c*), i, ii).

But, whatever criticisms may be made about the details of emergent evolution, it is certainly another of the modern

systems of philosophy which makes a complete break with "mechanism" (*e g* Broad, *ibid*, Ch II, *passim*; Woodger, p. 324) And I do not see why some form of emergence as regards inorganic nature might not be held in combination with such a general form of ordinary vitalism as that outlined in my concluding chapter.

"Teleology" requires some brief notice, for it is referred to in most works upon biological theories (*cf.* Woodger, *op cit*, p 429). As a general interpretation of phenomena which attributes purposiveness to Nature it is of great interest and importance for ontology and theology But vitalism is a biological problem and teleology, strictly speaking, a purely metaphysical one; so the metaphysical belief, which I happen to hold, is no argument for the truth of the scientific theory On the other hand, *The Fitness of the Environment*, to quote the title of L Henderson's book, is, to put it mildly, no argument against vitalism, though its author tried hard to make it so (*cf* II(*b*), iv, Haeckel, and II(*d*), ii).

But the term teleology can also be used in a narrower sense, concerning organisms This is Driesch's attitude in his *History of Vitalism* ("Critical Introduction," and p 176 *seq*) Though he is careful to speak of teleology in organisms as an analogy only, he contrasts dynamic or vitalistic teleology with the static or mechanistic variety But in his Gifford Lectures he avoids this phraseology (*op. cit*, pp. 243-4; *cf* III(*a*), ii, above). And Broad, for all his rejection of Driesch's arguments, agrees that organisms display "internal teleology" (*op. cit.*, p. 81 *seq*) Professor Boycott thinks teleology is admissible in biology and holds strong vitalist views (*Proc. Royal Soc. Med.*, Vol. 23).

Woodger gives examples of teleological descriptions of biological matters by Starling, Sherrington, J. S. Haldane, and E B. Wilson, and discusses the ambiguities involved in this procedure (*ibid.*, pp. 429-36). But, as his excellent summary shows, vitalism and mechanism are the only two types of theoretical biology yet devised and both involve the analogy of a humanly constructed machine. Vitalism allows for a mechanic. Mechanism forgets that a mechanic is

essential and only achieves being materialistic by omitting the "psychological origin" of the machine (*ibid*, pp 440-41; *cf* pp 451-3, also Berg's criticism of Loeb, p 101 above). Woodger's conclusion is, like that of Driesch, obviously vitalistic.

J S Haldane (English 1860-1936) —A mining engineer, a biologist, and a philosopher, the late J. S Haldane defies classification; but it seems best, on the whole, to consider his contribution to the vitalistic controversy here His principal publications on biological subjects are *Essays in Philosophical Criticism*, 1883, *Mechanism, Life and Personality*, 1913, *The New Physiology*, 1919. In 1908, when mechanistic views were strongly prevalent in biology, he addressed the Physiology Section of the British Association and advocated a strongly vitalistic view of the living organism and its essential properties such as heredity as "axiomatic" The problem of physiology was to discover the relatedness of details of structure and activity in the organism as expressions of its unity As a young man in 1885 he prophesied boldly that if "one of the two sciences [Biology and Physical Science] is swallowed up, that one will not be Biology" Sir F G. Hopkins, then President of the Royal Society, has lately criticised this view as containing an *a priori* element, which would hinder the advances achieved in such fields as that of biochemistry (*Advancement of Science*, 1933, p 16). But with all due respect to the scientific genius and the studious fairness of Sir Gowland Hopkins, I venture to think that Haldane's insistence upon the autonomy of life was a valuable contribution to biological thought in days when the opinion that biology was "nothing but" physics and chemistry was commoner than it is now

Woodger has pointed out that critics such as F. H. A. Marshall and D'Arcy Thompson misunderstood Haldane and so misrepresented his ideas and criticised them unreasonably. Haldane was asking physiologists to abandon their naïve materialism, not their methods of investigation, which, indeed, he used himself (*ibid.*, pp. 242-8) For such methods are quite compatible with Haldane's belief that "the inner nature of reality involves an integra-

tion, especially manifested in living things" (Whetham, p. 471).

(iv) *Epistemology: Naive and Critical Realism; the Vitalistic Conclusion*

"It is only within a comparatively recent period that epistemology has come to be recognised as a distinct department of philosophical enquiry" The term appears to have been first employed by Ferrier, who divided philosophy into two—epistemology, the theory of knowledge; and ontology, the science of what "truly is" (*Institutes of Metaphysics*, 1854; *cf.* G. D. Hicks, E B , 13, 448) But logically, though not historically, epistemology comes first, before entology, before the study of science or indeed of any other branch of enquiry.

The distinction between psychology and epistemology needs to be borne in mind, though the two are closely connected. Briefly stated, psychology studies the "subjective aspect of knowledge"; knowing, among other mental activities, as "a condition of the individual mind" can be examined introspectively and experimentally (*cf.* next chapter). Epistemology is concerned with the significance of what is known, or thought to be known As Hicks puts it: "When and where can we be reasonably assured that our representation of the real is true?" (E B , 13, 449a).

Bishop Barnes has summarised the impact of epistemology upon scientific thought very clearly. Naive realism—the idea that things are what they seem to us to be—has been accepted by us all at one time; and, it may be added, is employed in daily life by us all still with usefulness. Physical realism is a step forward; electrons, protons, and so forth are taken to be "real", as indeed they are in a fashion; yet this is only "the uncritical metaphysics of the trained man of science." Critical realism holds that reality is not normally or necessarily identified with any of our "constructs"; this corresponds with Kant's distinction between things as perceived by us and "things-in-themselves." "The average man of science has an instinctive dislike of metaphysics; he is irritated by the suggestion that his own assumptions, which seem to him so entirely reasonable, may not be able to

survive critical examination" (*Scientific Theory and Religion*, p. 552 *seq*)

As Whetham says, only in the twentieth century was it gradually realised that "philosophical materialism . . . cannot explain consciousness or stand for a moment against critical analysis" (*op. cit.*, p. 330).

This philosophic criticism, as I understand it, may be used to discredit, or at least to throw doubt upon, any scientific theory. But, principally, it destroys the validity of the determinist and mechanistic view of nature, which has ruled science, including the beliefs of many biologists, since the days of Newton and, especially, Darwin; though neither of those very great men can be held personally responsible.

For this criticism, like the scientific edifices of fact and theory which it seeks to regulate, is the product of the mind of Man (*cf.* next chapter, sec. iii, for evidence that "mind" is a fundamental "existent") As Hobhouse has said, all knowledge, of external things, causal relations, and so on, is mental; we have no absolute knowledge of external things (*Theory of Knowledge*, p 531); though there is "a (external) content present to an inward state" (*ibid*, p. 537). Mechanism, though essential from one aspect, is useless from that of the world whole (*ibid*, p. 581-2). But this mind is connected with the human body, which is related to the bodies of animals, even of plants in many respects, and its knowledge of external things is limited according to J Muller's principle of specific nervous energy (*cf.* II(*a*), iii). Therefore a valid theory of the science of organisms must take account of the science and metaphysics by which it is itself elaborated and regulated. Therefore biological science cannot be mechanistic, but must be vitalistic, using this term in a broad sense (*cf.* III(*d*), 2, and Ch. 2, v, below).

This I term the epistemological argument (III(*d*), 2, iv).

CHAPTER 2

# PSYCHOLOGY

## (i) *Introductory; epiphenomenalism; psycho-physical parallelism; behaviourism.*

As Whetham says (*History of Science*, pp. 323-8), there are two ways of studying the mind of man, the rational and the empiric. The former works deductively, starting from some metaphysical system such as Roman Catholic scholasticism or German materialism. The latter assumes no *a priori* position but progresses inductively, using, broadly speaking, two methods, introspection for one's own mental processes, observation and experiment on the minds of others and ourselves. It is therefore more truly "rational" than the first way; begun in Britain and France before the nineteenth century, it soon ousted the older one in all civilised countries. E. H Weber of Leipzig may be considered the founder of experimental psychology, though the term was first used by Fechner in 1860; Beneke, 1833, Lotze, 1852, and Wundt were among the pioneers (*cf* Merz. *European Thought*, ii, pp. 508-20)

Darwin, with *The Expression of the Emotions in Man and Animals*, commenced the alluring but difficult branch of comparative psychology. It is well to emphasise its difficulty in connection with the history of vitalism; for its exposition has varied from crude anthropomorphism, applied to lowly animals and even plants—*e g.* "roots seek this; Paramecia dislike that", and so on, to the fatuous mechanism which would account for all the apparently moral and intelligent behaviour of birds, Primates, dogs, and similar animals in terms of physics and chemistry (*cf.* II(*b*), iv, Loeb, etc., and "behaviourism" below). Broadly speaking, I consider that a tolerably accurate outlook on this fascinating question, still far from complete solution, is given by the organism concept discussed in Part III(*d*), 1. Always, however, there

remains the difficulty that, though through books, inscriptions, ceremonies, and conversation we can obtain direct interpretation of the thoughts of other persons, varying of course with their ability to express them and our ability to understand that expression, no such expression is possible from animals. They use and we can interpret actions and signs; some birds and higher mammals have different sounds and calls which undoubtedly express different emotions, and we can accept the statements of various authors that apes and monkeys go a little further than other brutes towards the rudiments of language Yet, here as elsewhere, the vast gap between the highest beast and the most simple of mankind, which Darwin always admitted, is tremendously apparent. So all further references to psychology in the present work are to human psychology (*cf* Woodger, pp. 460-61)

Largely owing to the influence of Darwinism, the general trend of psychology from about 1860 to 1900 was undoubtedly mechanistic (*vide* II(*b*), iv). Comparative psychology seemed to bring the human mind and spirit into the realm of deterministic animal psychology, for an explanation of instinct was supplied in the *Origin* (Ch 7), and Darwin indicated in the first paragraphs that he saw no sharp line between instinct and "judgement or reason" Experimental human psychology supplied explanations in the scientific style for reactions which seemed formerly to be obscurely and entirely mental"; and the great contemporary advance in general and nervous physiology had a similar effect, thus blending with the current materialistic philosophy (*cf* Ch 1, 1, above).

Applied to general psychology, this resulted in a revival of the epiphenomenalism of Cabanis by T. H. Huxley (*Science and Culture*, 1881), and, earlier, by Vogt and others (*cf.* II(*b*), v). According to this theory the essential thing is the physical, brain, change; all mental states or events are epiphenomena to this (*cf.* Woodger, p 54) This, Whetham says mildly, is "crude and unsatisfying; but it serves to focus the greatest problem which psychology hands over to philosophy" (*op. cit*, p. 328).

Bishop Barnes discusses this difficulty (*Scientific Theory and*

*Religion*, pp. 581-7), and rightly points out that a theory which ignores or relegates to the trivial the fundamental experience of will and existence must be wrong Yet he admits that we remain wholly ignorant concerning the correlation between mind and brain Epiphenomenalism, an epiphenomenon of the phenomenon of mechanistic biology, is, of course, wholly mechanistic Other grounds for rejecting it with Bergson and McDougall are given below in section iii.

But towards the end of the century a psychology developed which was anti-mechanistic W. Wundt, who opened the first psychological laboratory in 1879, and W James, whose *Principles of Psychology* was published in 1890, were among those who promoted the independence of psychology as "the Science of Mental Life" (James), which investigates "internal experience" (Wundt), (Woodworth, *op cit*, pp. 8-9). The evolution from mechanism to mind is also seen in H Munsterberg, while James Ward, F H Bradley, Dawes Hicks, and G. F. Stout contributed to the 'critical rejection of the 'mosaic psychology'" This mechanistic psychology was dominant at the end of the nineteenth century, especially in Germany; it represented thinking as a "stream of consciousness", composed of discrete "atoms of sensation", and was allied with that view that mental processes are explicable in terms of brain structure and changes which Head and Bergson have shown to be dubious or even incorrect. The modern German Gestalt school, with its emphasis on wholeness in perception, also contributes to the psychology represented by McDougall (*Outline of Psychology*, prefaces and Ch. I; *cf.* Woodworth, Ch. IV, and sec iii below).

Psycho-physical parallelism, unlike epiphenomenalism, assumes independence as well as parallelism in both physical changes in the brain and corresponding states or events in the mind. It is not, most probably cannot be, proved that every small brain change must be accompanied by a thought, nor that every mental change, as, for example, in "day-dreaming", involves a physical reaction in the brain (*cf.* Woodger, p. 467, Driesch, p 309) But many such phenomena are certainly paired; changes occur in the nervous

system, often accompanied by others in muscles or glands, and simultaneously corresponding ideas and images arise in the mind (*cf* Bergson, *Creative Evolution*, p. 276, etc ).

"Germs" of psycho-physical parallelism are found in Spinoza, Leibniz, Weber, and Wundt (*cf.* Whetham, p 327), and its primary root lies in the dualism of Descartes (*cf.* Whitehead, *Science and the Modern World*, p. 241). But Fechner (1801-87) was perhaps the chief exponent of a doctrine which "many psychologists adopted . . . as the working hypothesis in psychology" (E.B., 17, 755d). It is certainly useful for physiologists and psychologists to be able to "go ahead" in their respective spheres without having to consider possible restrictions from the other. But though this is convenient practically it is not wholly adequate Mental states are certainly affected by the condition of the body, by fatigue, good health, absorption of alcohol, poisons, and so on, while the evidence to show that mental or spiritual states affect the body is overwhelmingly strong. For example, sleep and digestion are often improved when mental worry, for instance over financial troubles, is removed.

As far as I am aware, this theory has been applied to Man only, never to animals Its bearing on vitalism is therefore limited; but it permits mental autonomy for Man and therefore opposes mechanism to some extent.

In the twentieth century various psychological schools have arisen; those which seem to affect vitalism require consideration.

"Behaviourism", which is not to be confused with the study of normal animal behaviour by E. S. Russell and other modern zoologists as considered in III(*d*), 1, below, is a form of "animal psychology", based, J B Watson claims, upon Lloyd Morgan's *Animal Behaviour* (1900), but developed mainly in the United States by Watson and others, his first papers on this attempted approach to psychology having appeared in 1913-14. It claims that psychology and behaviour can be studied like physics, biology, and other sciences; the method is to reduce all phenomena to a stimulus-response formula, so that sensation, will, conscious-

ness and other familiar psychological terms disappear; the good behaviourist finds no need for their employment The abolition of consciousness is in marked contrast to modern ideas in epistemology (*cf* Ch I, iv, above), but not more so perhaps than other aspects of behaviourism are to normal biological theory A long, discriminating account is given by R S Woodworth in *Contemporary Schools of Psychology* (Ch. III)

The whole theory is anti-vitalistic, and I agree with McDougall's description of it as "a most misshapen and beggarly dwarf" (*Outline of Psychology*, p ix), its *method*, however, is appreciated by Driesch (*Science and Philosophy of the Organism*, p 203)

(ii) *Psycho-analysis. and C. G. Jung.*

The *Concise Oxford Dictionary* defines psycho-analysis as "the psychology of Freud, Jung and Adler" . . . but here I use it for the typical doctrines of Freud I am not concerned with psycho-analysis practised as a method, its original meaning according to Freud (*Problem of Lay-Analysis*, p 315) But as an influential modern psychology, for and against which a mass of literature has been produced, it calls for some notice The essence of Freud's teaching can be gathered from *The Interpretation of Dreams*, an early work, and *The Problem of Lay-Analysis* (Brentano, 1927) This Austrian Jew is not only the father of this form of psycho-analysis, but its most prominent exponent, for Adler and Jung have parted company with him on many important points.

A concise summary of psycho-analysis according to Freud and his disciples is given by their British leader, Dr E Jones, in Benn's Sixpenny Series (1928) Here we learn that it has application to Medicine, Education, Anthropology, Sociology, Art, Literature, Criminology, Mythology, Religion, and apparently every branch of human activity except Science. But it concerns human mentality and actions and so has a bearing upon vitalism. The "censor" sounds vitalistic; but the whole collection of postulates and ideas which make up Freud's theory are based upon presumed precocious and perverted sexuality, notably the "famous Œdipus complex." "By the truth of this finding psycho-analysis

stands or falls," Jones says (p. 36); and I hold that it falls and falls heavily

Dean Inge has well said that if Freudism be accepted as a philosophy the world's great teachers, from Plato and Christ onwards, must be abandoned. An, in many ways, admirable medico-scientific criticism of Freudism was made in 1924 by Dr. P. McBride in *Psycho-Analysts Analysed* (Heinemann). Jung's deadly criticism of Freud's self-exposure is quoted below; with this I agree. We must also agree with McDougall that no system of psychology—or philosophy—should be founded on the abnormal; and Freud's own works show that his theory has been largely based upon experience with hysterical girls and neurotic persons of loose morals and aimless life.

A sound criticism of Freudism is given by McDougall in *Social Psychology* (Supplementary Ch. II). He shows that Freudians mistake manifestations of various kinds of affection and love as though sex-love were the only sort of affectionate emotion, which it certainly is not. McDougall thinks that the Œdipus complex may possibly be true for a minority, and he accepts, as doubtless we all do, the valuable idea of "sublimation", though the thing, if not that precise title, was known long before psycho-analysis arose. But on the whole, he rejects Freud's teaching for normal persons, for many admirably stated reasons; though he considers that Freud has rendered great services to psychology.

*Non tali auxilio* therefore is the attitude to be adopted as regards any justification of vitalism from this quarter.

A refreshing change from the, to my mind, preposterous and disgusting dogmatism of Freud and E. Jones is found in the latest book by Freud's former colleague, Dr. C. G. Jung (Swiss: 1875- ), *Modern Man in Search of a Soul* (Eng. trans., Kegan Paul, 1933). He thinks it best to abandon the claim that we can make statements about "the psyche" that are "true" or "correct"; all we know at present is what a few individuals say that they have found within themselves, and "the form in which they have cast it is sometimes adequate and sometimes not" "What Freud has to say about sexuality . . . incest and the like can be taken as the truest

expression of his own psychic make-up . . . had he critically examined his own assumptions, he would never have put his peculiar mental disposition naively on view, as he has done in *The Interpretation of Dreams*" (*op. cit*, pp. 133-5).

Jung points out Freud's inability to understand religious experience, and prefers "to look at man in the light of what in him is healthy and sound . . Every psychology—my own included—has the character of a subjective confession" (*ibid*, pp 135-6) He attributes a positive value to all religions and to biology and to scientific empiricism in general "In my picture of the world there is a vast outer realm and an equally vast inner realm; between these two stands man . . ."(*ibid*, p 137)

His Chapter IX has also many good things, including a vivid criticism of the falsity of materialism and its attempted explanation of the spirit of Man as an epiphenomenal growth from matter (*ibid*, p 204) He sees no absurdity in the idea that the "soul" lives in a realm beyond the body (*ibid*, p. 213).[1] Neither naturalistic nor spiritual interpretation alone is adequate for psychology "More than a few suicides in the course of psycho-therapeutic treatment can be laid at the door of such mistakes" (*ibid*, pp 217-18). " . the really modern man is often to be found among those who call themselves old-fashioned" (*ibid*, p 227)

There is much else in the book with which I heartily disagree; but much of the later part (Chs X and XI) can be put down to the effects of war and post-war upheavals upon a thoughtful man who for long had wandered in tortuous and uncertain intellectual paths. Here it is sufficient to note that the second leader in the psycho-analytical school has reverted to belief in the autonomy and independence of the human spirit, a view which stultifies mechanism as adequate for the science of human life and so at least keeps the door open for vitalism in general biology.

### (iii) *The Main Stream; Bergson; McDougall; Nunn and the Hormic Theory.*

It is now necessary to consider the main or empiric

[1] *Cf* Driesch "Mind and body are *two* different entities which are in interaction throughout a man's life" (297, *The Great Design*, Duckworth, 1934).

stream of modern psychological thought, and for this Bergson and McDougall may be taken as representative.

H Bergson (Anglo-Jewish, though naturalised in France: 1859- ) His views on psychology are represented in *Matter and Memory*, based on the fifth edition (1908) of *Matière et Mémoire* and revised by Bergson himself

"This book affirms the reality of spirit and the reality of matter" and "is, then, frankly dualistic" (Introd, p. vii). His standpoint is actually that of "moderate" or "new" realism (*cf* Ch I above) Objects as perceived by a human being he terms "images" (*ibid*, pp vii-viii), which of course is not the usual psychological use of that word.

The book consists of four chapters (298 pages), a "Summary and Conclusion" (34 pages) and an Introduction (11 pages). There is too much repetition, and in many places the translation at least is difficult to follow Opposition views are frequently stated and criticised and Bergson's views given in the same paragraph; sentences are often involved, with different subjects for different clauses; when a relative pronoun occurs, it is often hard to see which antecedent it applies to; so an effort is often required to discover which theory is referred to in a particular sentence.

There are, too, very few concrete illustrations, and this detracts considerably from the value of the book, and those given are much too elementary. For example, "action" is an essential element in Bergson's treatment of his subject (*e g.* in Ch I). "Our body is an instrument of action and of action only" (*ibid*, p 299); it stores up the action of the past in the form of motor contrivances (*ibid*, p 87); further, "of my past, that alone becomes image and consequently sensation, at least nascent, which can collaborate in that action . . . Memory actualised in an image differs, then, profoundly from pure memory" . . . "Pure memory is radically powerless" (*ibid.*, pp. 180-81). On page 206 he illustrates this by reference to animals; grass attracts herbivorous animals, whose immediate data are its colour and smell. But what of Man, or rather of different kinds of men?

The sight of a rabbit affects different men in different ways. Yet the only one that Bergson seems to consider is the elemen-

tary reaction of a sportsman or a famished person who reacts to this spectacle, as a carnivorous animal would, by bodily movements aimed at killing or capturing the rabbit But a vegetarian poet will behave quite differently, at least if he is well nourished, he may merely watch the rabbit or start a train of meditation. So too the evolutionist or naturalist A child will probably clap its hands with pleasure and slightly alarm the "bunny" An economist will normally make no movement but may begin a mental process of reflection on rabbits as food for growing populations. And so forth. Many philosophers consider only the reactions of the poet, the zoologist, or the economist; but that is no reason why Bergson's argument should apparently be based only on the more elemental responses of the shooter, the famished one, or the carnivore. This assumption that "action" of some sort is the basis for human activity, as for that of "living matter, in its simplest form" (*ibid* , p 67), is for me the most dubious part of Bergson's argument And it seems unnecessary if, as he says and I believe, spirit is an independent reality.

"We pass," he states, . . "from recollections to the movements which indicate their nascent or possible action in space" (*ibid* , p 88) He gives one illustration for this—the process of learning a lesson by heart; and the "action" in this case is said to be "all the articulatory movements that are necessary" even if the lesson is only repeated mentally (*ibid* , p. 91) This I find very feeble Why could he not discuss his own thought-processes and show how they are correlated with "action"? Children of Malay, negro and various less developed races do no doubt learn lessons in this way; too much so indeed. But I myself have often learnt scientific names for plants and animals from books with the uncomfortable feeling that if I had to pronounce them before a scientific meeting they might sound queerly, because I have often had no need to speak them aloud or consider their conventional sound.

But, Bergson also says, while the past may survive in motor mechanisms so that "the recognition of a present object is affected by movements when it proceeds from the object", there are also "independent recollections", and recognition

of an object is "by representations when it issues from the subject" (*ibid*, p. 87).

The general conclusion of the first three chapters is that the body (*i e.* of Man) is "always tuned towards action" and so limits "the life of the spirit" This Bergson considers "appears to be the fundamental law of our psychical life" (*ibid.*, pp. 233-4).

He then discusses the metaphysical problem of "the union of soul and body" The term "soul", we note, is not defined. He usually speaks of "spirit" and "matter" Pure perception places us within matter, which exists independently of the thinker or observer; while by or through memory we "penetrate" "into spirit" Introspection reveals both the union of matter and spirit and also their distinction (*ibid.*, pp 234-5)

Books have been written on Bergson's philosophy and it is not my object to try to compete with them in this short notice. What matters here is that one of the most influential and independent of modern philosophers believes, and gives good reason for believing, in spirit as well as in matter or physical organisms For him thought is no epiphenomenon or "secretion of the brain" Human life has an independent spiritual element, and cannot be explained entirely by chemical and physical forces The cerebral cortex does not store or secrete memories or memory-images, as materialists believe (*ibid*, pp. 81-2, 88, 132, 231, etc ) So whatever may be true of animals, especially of the simpler ones, Man at least must be dealt with on vitalistic lines This, in my opinion, Bergson demonstrates. And if for one mammalian species some sort of vitalistic hypothesis is essential, vitalistic doctrine may be the only correct one for other animals too, even though we agree with Bergson, as I consider we should, that there appears to be a spiritual or psychical gulf between Man and all the brutes of whose sensory-motor functions we have any knowledge (*cf. op. cit.*, pp. 93-4, 234; and III(*d*), 2, iv, below).

W McDougall (British: 1871- ).—In the two prefaces and Chapter I of his *Outline of Psychology* (Methuen, 4th edn., 1928) McDougall gives an able historical review of the con-

flicting theories which make some scientists wonder whether psychology is really a science; or, if it is, which of the many contradictory psychologies is the science and which are the equivalents of alchemy and astrology Thus McDougall himself only claims that his mature hypotheses are "crude foundations" for psychology, a modesty similar to that of Jung in his later mood McDougall started by regarding it as a science which could be dealt with by the methods of physical science, and his adoption of a vitalistic outlook is significant. Setting details aside, he points out "two principal alternative routes" for the student "(1) that of mechanistic science, which interprets all its processes as mechanical sequences of cause and effect, and (2) that of the sciences of mind, for which purposive striving is a fundamental category . . radically different from mechanical sequence" The latter method is now his own, and it agrees admirably with the general modern trend towards autonomy in biology as seen in Driesch, Durken, E S. Russell, and others (*cf* III(*a*), III(*d*), and Chapter 3 following)

He points out the danger arising from the tendency to "reify" all objects that can be denoted by a word of substantival form, which has led many psychologists to think of thinking as though it were a "stuff", hence came the development by B Russell and others of the idea that even the objects of nature may consist of the same "material", a view which McDougall rightly terms fantastic (*ibid.*, pp. 16-19). A vigorous criticism of "idea psychology" and the allied "mechanical reflex theory" follows, which shows the falsity of epiphenomenalism. The objections to the Reflex Theory are that it depends upon two assumptions. one that mechanistic physiology will eventually be able to explain organisms in terms of chemistry and physics; the other that it will be able to account for the relations between the facts of experience and the facts of behaviour. The unlikelihood of these assumptions being true is well and temperately indicated (*ibid.*, pp. 30-34).

By "acceptable hypotheses" he means working hypotheses which claim merely to be useful for the present, not ultimately true; and he considers the "old-fashioned" word "mind" suitable for the something which finds expression

in (1) the modes of individual experience, and (2) those of bodily activity which constitute behaviour Mind, he thinks, has the same nature, whether in animals, men, or superhuman beings; but its "structure", which, of course, is not material, varies in each individual. To substitute for this the brain limits freedom of thought, "ties us down to one kind of explanation", and is apt to blind us to facts of observation and bias our interpretation of facts But correlation between psychology and physiology is useful, and as much as possible should be interpreted in terms of nervous structure and function (*cf.* III(*d*), 2). He agrees with Whetham as to the methods of "empiric" psychology (*cf* sec i above).

Experience is a process, and implies a subject; this hypothesis is indispensable. "Purposive action is the most fundamental category of psychology" (*ibid*, p. 51), and free-will is not an illusion (*ibid*, p 447)

In Chapters II-IV he treats behaviour in the lower animals from the standpoint of E. S Russell, which seems to me the reasonable one (*cf* III(*d*), 1) Tribute is also paid to the classic researches of Fabre Tropism McDougall finds a "valid principle", but quite inadequate as a total explanation for animal behaviour on mechanistic lines His criticisms of Thorndyke's experiments are identical with those of E. S. Russell, namely, that they are too complicated for the animals and the conditions are unnatural His criticisms of Loeb's theories seem to me unanswerable (*cf* my II(*b*), iv), and his neglect of Semon's Mneme theory justified (*cf.* Ch. 3, ii, below).

While he has much in common with Bergson, he avoids the "master problem . . of the relation of mind to matter, of soul to body", only insisting that purposive action cannot easily be regarded as a form of mechanical process (*ibid.*, p. 450). And he accepts autonomy or "striving" for all animals, whereas Bergson's vitalism is largely limited to Man. He also disagrees with Bergson concerning Instinct. Bergson holds that instinct and intelligence are alternatives, typical, for example, of the insects and vertebrates respectively. McDougall considers, with Darwin and E. S. Russell, that there is no sharp line between these two, and that instinct, even in typically instinct-guided animals, may be

modified by intelligence on occasion, while instinctive action in the higher creatures, including Man, is the key to understanding their behaviour. He argues ably that the mechanists' view of instinct as only complex reflex action is incorrect (*ibid*, p 70, Ch III, etc ); while "Intelligence is the capacity to improve upon native tendency in the light of past experience" (*ibid*, p 71).

Space does not permit consideration of the details of McDougall's psychology, but it is sound in principle, for it is based not on abnormal pathology or laboratory experiments on maimed animals but largely upon observations in the field or on healthy tame beasts such as dogs, pigeons, and Kohler's famous chimpanzees His *Outline* illustrates forcibly the decline of the late nineteenth-century psychologies and the inadequacy of such later developments as tropism and behaviourism. But probably McDougall pushes explanation of elementary functions such as sneezing and urinating by instinct too far (*ibid*, pp. 163-5)

The Hormic Theory. McDougall agrees entirely with Sir Percy Nunn (*Education · Its Data and First Principles*) that animals cannot be "explained" in terms of chemistry and physics, but that even "the humblest creature is autonomous"; that there is a striving towards individuality, seen best in Man but also in all animals, and that this "urge" or "drive", the *élan vital* of Bergson, *libido* of Jung, or conation of psychologists for conscious beings, is purposive, and may be termed "horme." This is of course another expression of belief in vitalism as regards animals and Man, though the hormic theory is inapplicable to plants (*Outline of Psychology*, pp. 71-3); McDougall explains it in Man as due to innate instinctive impulses which "determine the ends of all activities and supply the driving power" His psychology is essentially hormic (*ibid*, pp. 218, 370-5); "we cannot explain the generation of Mind out of something that is not Mind"; and all the often supposedly separate functions of mind, such as memory, conation, and so on, "are involved in the simplest mental acts" (*ibid.*, pp. 308-9).

His emphasis on the conative element in memory, imagination and other mental operations is suggestive of Nietzsche's will-to-live (*ibid.*, Ch. X, pp. 317-20).

McDougall traces his treatment of emotional expression back to Charles Darwin (*ibid*, pp. 321-3), and, with Professor W. B. Cannon (*Bodily Changes in Pain, Hunger, Fear and Rage*; New York, 1915), appears to turn the flank of mechanistic explanations based on endocrine secretions by attributing the action of the hormone to a nervous impulse. But elsewhere (his Ch. XIII) he very properly admits the strong effect which the body exerts upon mental temperament, as well as the reverse effect

His theory of play is allied to Wallace's great idea that healthy animals, at least in the groups best provided with means of perception and locomotion—such as normal insects and the higher vertebrates, possess an excess of energy; though McDougall typically regards this as "nervous", he also terms it "vital" energy (*ibid*, pp 171-2) His discussion of instincts would be simplified, I consider, if he allowed for an education instinct as an extension of the "parental instinct" in animals which tend their young, though he does admit the importance of "learning" (*ibid.*, p 184)

Other respects in which he agrees with Wallace are the attribution to the animal kingdom of what he calls horme or autonomy, Wallace "consciousness", and the likelihood that organic evolution is due to Mind as the creative agency (*ibid.*, p. 448; *cf.* II(*d*), ii, above)

He shares with Driesch the belief that "Intelligence . . . directs forces or energies without being itself a force or energy" (*ibid.*, p 440). His powerful advocacy for vitalism in animals and Man is clinched by the remark that belief in "strict determinism" in a man who makes strenuous efforts is a mild symptom of mental disorder (*ibid.*, p 448).

In conclusion it should be gratefully noted that, unlike many psychologists, McDougall writes simply and clearly, though sufficiently profoundly.

In *Biology and the Sciences of Life* (1934), he summarises "the evidence during more than forty years of cold and sceptical enquiry" which, as indicated in his earlier *Body and Mind*, has led him to believe that "man . . . is more than a machine, and more than a mirror that reflects the world about him" (*op. cit.*, pp. v and vi). This is the view of Bergson and of Singer (*History of Biology*, p .429) and others, and

forms an argument for vitalism which can be termed the Autonomy of Mind (*cf* III(*d*), 2, iv, No. 11).[1]

One of the strongest arguments for the independence of mind or spirit is, in my opinion, to be drawn from the palpable difference achieved by the evolution of a group spirit, will, or *esprit de corps* in such typical human groups as a crew, a battalion, a school There may be two of these units equal in all physical respects—numbers, weight, racial, type, quantity of food eaten and physical energy available, and so on Yet the one may be a useless, spiritless mob, the other a unit capable of achieving far more both spiritually and physically than the first because it is disciplined and inspired. This distinction is due not to any physical cause but to the action of the mind of a leader or ruler, or, possibly, to the co-operative wills of the individuals. Similarly, the ideas of great historical characters, such as, for example, Alfred the Great, Wesley, Napoleon, Livingstone, Mussolini, have achieved results which it is preposterous to attempt to evaluate in terms of chemistry and physics But these arguments are not used by our psychologists, who are much too prone to individualism.

(iv) *Eddington and the mind-brain connection*

The notable contribution of Sir Arthur Eddington to the indeterminacy controversy is dealt with in the following chapter Here we note his tentative excursion into the supreme problem of psychology—the mind-body relation, given in *The Nature of the Physical World* (*Volition*, pp 310-15).

How is it that "the mind gets a grip on material atoms so that movements of the body and limbs can be controlled by its volition"? For "we may now feel quite satisfied that the volition is genuine", as I think the preceding reviews of modern philosophy and psychology have demonstrated. Also, Eddington's argument against the universal validity of the materialist reflex view of human action (*op. cit*, Ch. XIV, especially pp 311-12) deserves careful consideration.

He believes in direct action by the mind upon "certain atoms or elements of the physical world", in the brain; but

[1] *Cf* J W Dunne's argument that the soul of Man is immortal (*The New Immortality*, Faber & Faber, 1938).

rejects the view that there are "certain key-atoms" which alone are thus affected to produce changes in the material world His solution, which I find outlined with less than his usual clarity, is that "in the physical part of the brain immediately affected by a mental decision there is some kind of interdependence of behaviour of the atoms which is not present in inorganic matter"' That is, he finds himself forced to admit a distinction between "inorganic and organic (or, at any rate, conscious) matter", though he admits its "seriousness"; but interference with the probability of the undetermined behaviour of an atom seems a less drastic interference with "natural law" than other modes of mental interference suggested (*ibid*, pp 313-14). I would suggest that Sir Arthur makes unnecessary difficulty by putting "natural law" in antithesis to his suggested "mental interference"; if "mental interference" be also a "natural law" in its appropriate "natural" sphere this difficulty is abolished, or at least reduced to the familiar plane on which one "natural law" gives way to any stronger one with which it comes in conflict For instance, a mixture of hydrogen and oxygen molecules will increase its volume or pressure according to a "natural law" until under another such "law" they turn into water or steam and the operation of the first "law" is modified.

Whetham (*op. cit*, p 476) takes what is doubtless the usual view, that, while Eddington's suggestion deserves all respect, "the problem of the mechanism of the connection between mind and brain is one of surpassing difficulty . . . it may be better to leave this problem in its earlier form." He considers physical science and psychology as separate aspects of "reality"; but the latter must recognise "aesthetic, moral and religious emotion" as part of its data.

Anyhow, both these great physicists recognise the independence of the mind, and Eddington's reluctant admission of a distinction between organic or conscious and inorganic is definitely vitalistic, and in agreement with the vitalistic recognition of the independence of mind by our leading "empiric" psychologists.

CHAPTER 3

# MODERN BIOLOGICAL THEORY

## (i) *Chaos in Biology.*

SIR JAMES JEANS, addressing the British Association in 1934, compared the growth of science during the previous fifty years to that of a "main edifice . . . increasing in extent, dignity and beauty", but the department of theoretical physics to a building "brought down in ruins by a succession of earthquake shocks . . . because it was not built on the solid rock of ascertained fact but on the ever-shifting sands of conjecture and speculation."

Faced with this candour, what is an honest student of biology to say when he contemplates his own portion of the "edifice" of science? A vast amount of research has been done, though vast fields remain little explored, especially in outdoor studies in the tropics. Great practical results have been obtained in certain departments, such as the physiology of hormones and vitamins. But as regards theory I see little but chaos in Biology (*cf.* Woodger, p. 476, bottom).

Doctors E W. MacBride and C. Singer maintain that natural selection merely means that "the survivors survive." Dr. R. A. Fisher, supported by J B S Haldane and others, claims that the "overwhelming" strength of natural selection is demonstrated by appropriate mathematics. Julian Huxley, the late Sir Arthur Thomson, and others support natural selection on biological grounds, correctly, I believe, at least to a certain degree.

Lamarckism is extolled by MacBride, faintly supported by Broom, and opposed by most biologists, Darwinian or otherwise.

"Darwinism" in general is supported by Poulton, J. S. Huxley, J. B. S Haldane, T. H. Morgan, and many others. It is still probably the orthodoxy of the rank and file of biolo-

gists throughout the world. It is, however, opposed by the Lamarckians, by Berg, Driesch, and many others. In *The Coming of Man* (Witherby, 1933), Dr R Broom gives an independent and stimulating view of the facts and of the incongruous theories competing to explain them.

Professor L. Berg in *Nomogenesis* claims that evolution must have been, not from one or a few forms but, from "thousands", the ultimate origin of which he does not discuss. Neo-Darwinians follow Darwin in thinking that evolution must have been from a few forms or one, though they disagree completely on such a question as the possible origin of the Chordata from a primitive form of some lower phylum. Though biogenesis is admitted by all as a present fact, the mechanistic school still hopes to discover abiogenesis in the future—or possibly in the extremely remote past (*cf.* II(*c*), iii). Even the scope of biology is disputed by J. Needham and some biochemists, who would make it a department of physics or chemistry (*cf.* next section); though few if any biologists, in any true sense of the word, can agree to this (*cf.* III(*d*), 2).

To this selection of conflicting biological theories may be added the vitalist-mechanist controversy, together with the modern alternative that they are both right—or wrong—and will in future disappear in a unity in which biology will be swallowed up in a larger and brighter physics (*cf* III(*d*), 2, v); or, as J. S. Haldane prophesied, with physics inside biology. To sum up, little agreement exists among biologists except about biogenesis, so far as we *know*, and about some kind of evolution in some way or another (*cf. Creation by Evolution*, various writers, MacMillan Co., 1928; and *Religion and the Sciences of Life*, p. 7).

Jeans claimed that his ruined physics department was capable of being rebuilt as a mathematical structure. There seems little chance yet of co-operation among the bricklayers in the theory-of-biology mansion; the brick that one lays so carefully often displaces the favourite brick of his neighbour. Thus, in the theory of evolution there is evolution but not necessarily progress. Perhaps the metaphor suitable for physics is inadequate for biology, and we should be content with the relative freedom of an organism—or a colony of

such—instead of seeking to build a mathematically correct "edifice"

We must now consider the effects of recent developments in biological theory upon the history of vitalism.

(ii) *Modern Mechanistic Biology*

The mechanistic tradition of the later part of the nineteenth century (*cf* II(*b*), iv, v) continues in certain quarters. Thus, as late as 1911, Sir E Sharpey-Schafer in his presidential address to the British Association, claimed that "the combination of these elements into colloid compounds represents the physical basis of life, and when the chemist succeeds in building up this compound it will, without doubt, be found to exhibit the phenomena which we are in the habit of associating with the term "life" But in 1933 his successor, Sir F. Gowland Hopkins, remarked that this claim "might have seemed to encourage the biochemist, but the goal suggested would have proved illusive, and the path of endeavour has followed other lines" (*Advancement of Science*, 1933, p 4). And no man is better qualified to speak of the realities and potentialities of biochemistry than this distinguished President of the Royal Society (*cf* III(*d*), i)

Nevertheless, the mechanist view is nowadays largely maintained by biochemists and physiologists It is that physics and chemistry are sufficient for the understanding and explanation of all organic phenomena, even those of the mental and spiritual life of Man, at least as far as "science" is concerned

This position is stated with much repetition by J Needham in *The Sceptical Biologist* (1929). The mechanical theory of life, he says, is a method of universal application, applicable even to the mind of Man; vitalism, whether old or new, is erroneous and unscientific (*op. cit*, pp 27-30, 6-23, 90-130); Driesch, Haldane and J. A. Thomson are as wrong as Stahl or van Helmont. Eddington is quoted, most incompletely, as stating that science is concerned only with the metrical aspect of the world; complete scepticism about the enigmatic background of the metrical world is then postulated for science, which, therefore, has no room for anything but a "legitimate materialism" (*ibid.*, p. 119). So neo-mechanism

is the only proper philosophy for scientists; and, "if biology is to be a science, in biology it must reign" (*ibid*, p. 39).

But full credit must be given to Needham for his philosophical treatment of this definition of science. Neo-mechanism is not the same as the scientific naturalism of T. H Huxley and other Victorians, which claimed to account for all phenomena in a materialistic way; it is universal in applicability and restricted in essence (*ibid.*, pp 38, 39, 243, etc.) He recognises that science gives but one, imperfect, view of things as they are; and that other views—the historical, religious, artistic and so on—are equally valid and equally necessary (*ibid*, pp 25-6, 86, 135) That this attitude is correct many students of biology will agree, as I do. But it is a matter of philosophy, not of biology.

Here we must consider the assumption that neo-mechanism is the only valid biological theory. Needham endeavours to justify it by defining science in such a way that the closed cycle of metrical readings will include biology as well as physics. Science is and must be "materialistic" because "we" make it so, "it is deterministic, but we unconsciously reject what would make it otherwise" (*ibid*, pp. 65-6). "Science is essentially analytical, mathematical and mechanical" (*ibid*, p. 245). Whatever can be weighed and measured forms its subject-matter; "elements of experience which cannot be so discussed are left on one side until they can" (*ibid*, p. 136). But Needham lets the vitalistic cat out of the mechanistic bag by his grotesque assertion that the concept of organism is "quite foreign to the scientific mind", though "proper to the domain of the philosophy of science" (*ibid.*, pp 84-5) The minds of most biologists do not reject the fundamental idea of organism or of its development (*cf.* III(*a*), III(*d*), and secs. iii and v here). And science should not contradict other methods if they are valid, as Needham says they are (*ibid.*, p. 66). The statement that "practically all working biologists admit the scientific approach stands committed" (*ibid.*, p. 135) to his own mechanistic theory of life, like many more before it, is ludicrously false, excusable only by the illegitimate extension made by Needham of physics to include all science. But vitalists refuse to be humbugged in this way, or to reject,

deliberately or "unconsciously", what does not fit in with arbitrary, *a priori*, limitations of science. They adopt experimental embryology with Driesch, the organism method with Russell, or empiric psychology with McDougall, find that certain events occur, and adapt their theories to these phenomena instead of cutting out the phenomena that do not agree with theories elaborated in other branches of science.

Needham admits that philosophic materialism is an epiphenomenon, as Ward showed, and behaviourist psychology erroneous (*ibid.*, pp. 239-40); yet mental states are epiphenomena for neo-mechanists, though little has been done yet for mechanistic description of cerebral processes (*ibid.*, pp. 142-4), and "anthropos" must be "deanthropomorphised" as the subject of scientific thought (*ibid.*, pp. 217-18)!! And so it goes on. Science of the neo-mechanistic brand being "a tremendous edifice of supposals or fictions", biologists can do nothing unless life is merely an equilibrium of proteins, fats, and so on (*ibid*, p. 252). Yet perhaps the kidney and brain are not only mechanical. "The biologist is not committed to any opinion as to what his animals are in themselves" . . . (*ibid*, p 253). To this and more like it vitalists, and many biologists who prefer to be unlabelled, would reply that they are content to believe animals are animals, and may be legitimately studied as such, whole and alive, as well as in little bits (*cf.* Driesch, *Science and Philosophy of the Organisms*, p. 3.)

In fact, a useful definition of modern vitalism would be that it is all that neo-mechanism as expounded in *The Sceptical Biologist* is not.

The Mnemic Theory. Originated by Hering in 1876 and accepted by Haeckel, this was elaborated by Richard Semon in several books, of which *The Mneme* (Eng. trans., 1921) is chief. It is in many ways confusingly written, and suffers from considerable repetition and a flood of peculiar terminology.

As Driesch says, *The Mneme* is "expressly conceived in a mechanical sense" (*History of Vitalism*, pp. 161-3). Semon criticises vitalism, and explains that his way of regarding heredity and similar problems "renders unnecessary vitalistic principles" (*ibid.*, pp 277, 291-2 and 14). This is also the

*ne plus ultra* of anti-vitalistic prejudice posing as science; for Semon's entire theory, such as it is, is based upon an "irritable organic substance", of which the properties are different to those of inorganic matter (*ibid*, pp. 10, 14, 288, 292). But it is just this assumption which Semon makes so glibly that is for Driesch, Wallace and others, including myself, part of the quintessence of vitalism So, quite unconsciously and unintentionally, Semon's teaching was fundamentally vitalistic; though all his obscurely expressed details were supposedly mechanistic in trend! But the whole book is a muddle Verworn rejects the whole theory contemptuously (*Irritability*, pp 16-17)

The mechanistic view in biology has also been maintained well into the twentieth century by the famous Russian physiologist Pavlov (1849-1936), some of whose experiments are familiar to all zoologists It all sounds very simple and very scientific Definite experiments are made, and not with pithed frogs or fragments of nerve and muscle but with real live mice, dogs and so on Preconceived theories are, supposedly, set aside, and we observe experiments and introduce no superfluous conceptions So we find that certain actions —secretion by salivary glands, for instance—are induced by certain stimuli Action follows stimulus, so it is called reflex action; given the condition, the conditioned reflex follows. So, we are led to believe, all actions, including those which superficially appear to indicate autonomy, are simply conditioned reflexes And it is easy to correlate this with the findings of biophysics and biochemistry, especially if the naive restriction of biology claimed by Needham and his friends is accepted. "Behaviour," Pavlov states, "is therefore completely determined by the stimuli and by the connections already existing in the nervous system or built up during the formation of conditioned reflexes" (*Advancement of Science*, 1934, pp. 94-5; *cf.* II(*b*), iv, *re* Haeckel, Loeb, etc.).

But this does not agree with the findings of Driesch (behaviouristic) and Bergson and McDougall (psychological) concerning human action (*cf.* III(*a*), ii, and III(*b*), 2, iii). More important is the revolt against Pavlovism by modern workers on animal behaviour. Durken's ideas are dealt with

below in section v. Lashley (*Brain Mechanisms and Intelligence*, Chicago, 1929), Koffka (*The Growth of the Mind*, 2nd ed, 1928), and the Gestalt psychologists are quoted by E. S. Russell (*The Behaviour of Animals* and *Advancement of Science*, both 1934) to show that, while some reflexes, simple or conditioned, undoubtedly occur, as medical men have known for generations, it is against all knowledge of wild animals to attempt to explain their actions in such rudimentary terms I would myself go further than Koffka, who claims that action comes to an end when *needs* are satisfied (*op cit*, p. 103), and say with those great naturalists Fabre and Wallace that much of the action of insects and birds, at least, is joyously superfluous to their "needs."

(iii) *Neo-Darwinism and rival theories.*

In certain respects the neo-Darwinism of the twentieth century has changed from the *Darwinismus* of the preceding generation (*cf* Part II(*b*), iv; (*d*), iii, etc ), but in others it is much the same.

In 1931 the veteran Sir Edward Poulton stated that "the theory of evolution as it was held forty years ago, and, I may add, very nearly as it is held to-day," was based on the "gradual summation" of small variations, the selection and accumulation of which, "as a result of environmental conditions", led to "specific changes" (*Advancement of Science*, 1931, *Zoology*, pp 9-10) Natural selection was defended, the concluding section of an address rich in historical matter being devoted to "recent work with a direct bearing on Darwinian evolution . . the researches upon Mimicry and allied subjects" . . (*ibid*, p 12 *seq.*). As usual, supposed mimicry in butterflies played a large part in this treatment (*cf* p 202 below). Finally he quoted the late Earl Balfour's opinion that "nothing suggested in recent years had replaced or modified the Darwinian theory of evolution", in which sexual selection also plays a large part (*ibid.*, pp. 25, 26).

This is the general conclusion of neo-Darwinians such as Julian Huxley and E. S Goodrich (*cf.* art. *Evolution*, E B., 1930), though J S. Huxley admits that orthogenesis "may play some part" (*loc cit*, p. 916).

A thoughtful account of *The Causes of Evolution* was pub-

lished by J B S Haldane in 1932. He admits that, though evolution is well established, "its possible causes . . . are certainly debatable" (*op cit*, p 3) "The world is full of mysteries", of which "life is one. . . It is not the business of an evolutionary theory to explain these mysteries" (*ibid.*, p 6). All this is a pleasant change from Haeckelian solutions of these mysteries or "riddles."

He too rejects Lamarckism (*ibid*, pp. 10, 20), but pays tribute to Berg for "the best anti-Darwinian book of the century" (*ibid.*, p. 10); and admits orthogenesis to some extent (*ibid*, p 151) Much of his book is an exposition of modern genetics; but he does not think that Mendel's "atomism" has reduced genetics to biophysics (*ibid*, p. 60). He regards natural selection as a *vera causa*, giving instances which I think are perfectly sound Yet, "neither selection or mutation alone can furnish a basis for prolonged evolution. Selection must wait for suitable mutations" (*ibid*, p 110). But it is a fallacy to think that natural selection always makes an organism fitter in its struggle with the environment; it may be helpful to the individual but "ultimately disastrous for the species"; so he explains monstrous horns, spines, and such like (*ibid*, p. 119 *seq*) But Wallace, Broom, and the orthogeneticists see no need to evoke natural selection here, and I consider they are right and Haldane wrong; and similarly as regards colours and songs of birds (*cf* Haldane, pp. 126-8).

He is, however, cautious about "Darwinism", as Broom points out (*Coming of Man*, p 20). "To sum up, it would seem that natural selection is the main cause of evolutionary change in species as a whole"; but we are still in the dark about variation (Haldane, p 142), and so to a large extent about the "Causes of Evolution."

But belief in organic evolution is no longer synonymous with Darwinism.

Lamarckism is stoutly upheld by MacBride, slightly so by Durken and McDougall, and sympathetically considered by Broom and other biologists. But though Darwin accepted it to a limited degree in the later editions of the *Origin*, it is rejected by neo-Darwinians, as has been shown. It is also rejected or, at the most, regarded as unproven by many

leading non-Darwinians. Driesch's rejection of it and of Darwinism has been described (pp 147-8 above) Berg considers Lamarckism to possess "no importance" apart from nomogenesis (*op cit*, p 14) Even Broom has little hope that the matter can be proved by experiment; D. M. S. Watson thinks that both Lamarckism and Darwinism "rest on a most insecure basis" (*Coming of Man*, pp. 20-21). Durken, though sympathetic, insists that embryology does not support the historical form of this theory (*Experimental Analysis of Development*, pp. 205-7).

Lamarck himself believed in abiogenesis, and wrote largely in a mechanistic way But his theory is in fact vitalistic, for it assumes in animals, though not in plants, an innate power of modifying their structure, not by direct response to external stimuli but indirectly owing to "needs" arising in the animal in a psychic way (*cf Zoological Philosophy*, pp 108, 112, 119-26, and Rádl, *History of Biological Theories*, pp. 271-4) But Lamarckism is so slightly held by modern biologists as a class that I do not adduce it as a support for vitalism here

Orthogenesis was first suggested by Eimer, whose ideas have been considered in II(*d*), iv, above Professor D M. S. Watson and some other biologists maintain to-day that orthogenesis gives the correct interpretation of many zoological facts Within an animal group many variations are found; but they are variations within limits; for instance, the limbs of vertebrates vary enormously in birds, mammals and other classes, but always there is the basic plan of two pairs, anterior and posterior Other examples occur in insects and other great animal groups Orthogenesis is often independent of environment, and its cause seems to lie within the animal; so development may have an adaptive significance or not (*cf.* E B., 16, 942). There is a close connection between orthogenesis and Berg's nomogenesis.

Meanwhile, since Darwin's day, stupendous difficulties have arisen to oppose the original thesis of an evolution from elementary primitive forms to later, increasingly complex ones This is seen extremely clearly in the plant kingdom The first general sequence observed—giant Lycopsida in the Coal Measures, next Gymnosperms, then Angiosperms

in the Chalk and Tertiaries—indicated a straightforward development from lower to higher. But increased knowledge has destroyed the basis for this simple creed.

An admirable summary of the botanical difficulties concerning evolution was given by A G. Tansley in 1923: "On the whole, the search for common ancestors as such has been disappointing"; Pteridosperms and Ferns "at all times show themselves perfectly distinct (D H. Scott, *Nature*, 11th Nov., 1922)"; and so on (*Advancement of Science*, 1923, *Botany*, p. 4). Tansley quotes Professor Seward's doubt that "the unfolding of terrestrial life is capable of being represented as a single chain"; and his suggestion that, despite "continuity in some degree", "plant life . may best be represented by separate and independent lines of evolution or disconnected chains which were never united . it would almost seem that 'missing links' have never existed" (*ibid.*, pp. 5-6).[1] Dr Scott in *The Origin of the Seed Plants* (p 227), infers that these "constitute an independent phylum, of equal antiquity with any . . Vascular Cryptogams"; Tansley suggests that there is insufficient evidence for regarding Seed Plants as a *single* phylum (*op cit*, p 7)); and follows this up by criticism of the whole neo-Darwinian position, and a valuable reminder of the unwholesome effect that specialisation has had upon botanical science as a whole—a criticism which applies equally to zoology Dr. F O. Bower, F.R.S, agrees with these other leading botanists (*Evolution in the Light of Modern Knowledge*, 163-7).

What Tansley suggested for plants has been urged much more forcibly for animals by Berg in *Nomogenesis* (1926). Berg at first appears to be definitely anti-vitalist; hesays that vitalism in general and the distinctive theories of Hartmann, Driesch and Bergson lead us nowhere towards the "full comprehension" of "purposive adaptations." But he is an equally strong opponent of abiogenesis (*op. cit.*, p. 2; *cf.* my II(*c*), iii), and of a mechanistic conception of life based upon it; he himself has no explanation to offer concerning his own theory, *i.e.* nomogenesis (*ibid*, pp. 2, 6); and he adopts the views of C. Bernard and H. Spencer that "*purposive structure* and action are thus *a fundamental property of the living being*"

[1] *Cf.* Sir A. C. Seward, F.R S., in *Nature*, 20th Aug 1938, p 311

(*ibid.*, p. 8). In fact, his book definitely supports the general vitalist view, that life is autonomous and inexplicable in terms of "physico-chemical terminology", expressed at the conclusion of the present volume (*cf* III(*d*), 2).

His "nomogenesis" is allied to orthogenesis; "every group of organisms in . a definite period attains its optimum, after which, *obeying certain internal impulses* [italics mine—L. R. W ] . . . it becomes extinct or is relegated to a secondary position, yielding its place to others"; as, for example, mesozoic gymnosperms and reptiles He admits that "the struggle for existence *may* contribute its quota to the final extinction", but states that the principal authorities in palaeontology are adverse to the struggle as a factor in evolution (*ibid*, pp 70-1). But Berg's arguments concerning Ammonites and some other animals are severely criticised by MacBride (*Eugenics Review*, April, 1927)

The theory of selection, he claims, is refuted by ontogeny (*ibid*, pp 132-3). "We are generally unable to tell whether we have to do with homology or analogy . . . all we know . . . refers solely to resemblances and differences (*ibid*, p. 162). "Among insects we meet everywhere with a convergent development" (*ibid*, p 167) The successive manifestations of new characters are governed by law, and there is no place for chance in evolution; "evolution is nomogenesis, or development determined by law" (*ibid*, pp. 154-5). "To support the view that animals descended from four or five progenitors is now impossible; the number of the primal ancestors must be computed in thousands or tens of thousands" (*ibid*, p. 358; *cf* pp. 169, 189, 197, 205) In this connection, as Berg opposes the favourite mechanistic fancy of abiogenesis, it seems to me impossible to avoid the conclusion that these numerous "primal ancestors" must have arisen by special creation. How else could they exist if they do not arise abiogenetically or by descent from earlier forms? But, of course, a professor in Soviet Russia might find it inconvenient to refer to a Special Creator, even if he so desired.

The particular case of coloration in butterflies is advanced as a proof of mimicry and natural selection by Sir Edward Poulton, Dr. Hale Carpenter, R. A. Fisher, and other neo-Darwinians. Poulton's belief in butterfly "mimicry" as an

argument for natural selection is well known (*e g* art. *Mimicry*, E B, 1930); even though, in some cases, "the abundance of mimetic forms when their models are rare, and still more absent altogether, does make it difficult to feel confident that *Natural* selection . . has always been the cause" (*Advancement of Science*, 1931, *Zoology*, p 20), his main belief is not affected, though it was largely based on his inability to think of an alternative explanation (*ibid*, p 24; *Colours of Animals*, 1890, p 231) However, R C Punnett argues, in *Mimicry in Butterflies*, 1915, that so far as butterflies are concerned this argument is unsound Berg regards mimicry as a particular case of convergence (*op cit*, p 337). But though I incline to the neo-Darwinian views about mimicry in many insect classes, and especially to Wallace's ideas about recognition marks and other types of colour in animals (*Darwinism*, Chs. VIII, IX, X) my study of the literature of bird attacks on butterflies and the observations of myself and all other observers in Malaya (see Bibliography—Wheeler) lead me to agree with Punnett and Wallace as regards upper wing colours in opposition to the orthodox Poulton school. Birds in Malaya at least do not attack butterflies to any appreciable extent, so there is no cause for upper wing colours to be affected by natural selection there, nor, probably, much elsewhere

A modern outlook upon evolutionary problems is that of Professor F Balfour-Browne, who delivered the presidential address to the British Association Zoological Section in 1935. The struggle for existence, he rightly states, is "beyond dispute"; but it may not be so exacting as Darwinians suppose, and "Natural Selection plays a much smaller part in the origin of species than has been claimed for it" (*Advancement of Science*, 1935, pp. 64-5, 76, 63). He recognises that mobile animals, unlike plants, can exercise a good deal of choice as regards their environment, a vitalistic matter which has often been overlooked (*op. cit.*, pp. 67-8).[1] Orthogenesis is recognised, and the possibility of acquired characters, also the importance of mutations and of chromosomes (*ibid.*, pp. 77-8).

So we find that the rigid "*Darwinismus*" of the nine-

[1] *Cf* Elton, *Animal Ecology and Evolution*, p 51.

teenth century is rejected by most biologists to-day, as is Lamarckism Orthogenesis is certainly capable of vitalistic interpretation; so, obviously, is Berg's nomogenesis Neo-Darwinism, as described by J B S Haldane, seems to me to be so too; orthogenesis is admitted to some extent, there is nothing mechanistic about mutations, and nothing in his book contradicts the vitalistic conclusions drawn from development by Driesch and Durken As regards the human mind or spirit, Haldane admits the possibility of origin by a great "mutation", though he appears to think it unlikely (*op. cit*, pp. 154, 168, and 4), and "mind" is not likely to be accounted for by natural selection (p 161), which is a revival of Wallace's belief In some such recognition of the partial value of rival theories as is shown by Balfour-Browne, J B. S Haldane, and Bergson (*Creative Evolution*, p 89) lies a way out of the present chaos; and it will not be tied to mechanism [1]

(iv) *Biochemistry; Enzymes and Hormones*

However chaotic modern biological theory may be, remarkable progress has been made in the discovery of new facts, notably in the study of enzymes, vitamins and hormones.

This is how Dr. H. J Phelps explains progress in enzyme research for non-specialist science masters:

"A great majority of the chemical reactions taking place in the living cell must be promoted by highly specialised catalysts called enzymes. As our knowledge . . has expanded, it has been found necessary to postulate (!) an ever-increasing number of enzymes, each of which is the specific promoter of a particular type of chemical change, and in many cases is specific for a single chemical reaction of one particular chemical compound" And so on. (*School Science Review*, October 1933 )

It may seem a far cry from this to van Helmont's "blas" presiding over the chemical changes in the body or to Stahl's idea that chemical changes in organisms are different from the analogous ones that occur in inorganic matter, and modern scientists, of course, feel that the enzyme conception is

[1] *Cf* Discussion on Natural Selection, *Proc Roy Soc B*, CXXI, 1936-37 ; and on Mechanism of Evolution, Brit. Ass , summarised in *Nature*, 17th Sept. 1938.

more scientific It appears to account in a material, chemical way for so much without "postulating" metaphysical forces. Still more so when it is also "postulated" that the occurrence of the enzymes themselves is due to the existence of more primitive "agents", the "zymogens", though I have not learned whether the occurrence of the zymogens themselves at the right time and place is "explainable" by the preliminary occurrence of what might be termed "protozymogens" But the whole conception of enzymes, whether they be well known catalysts or merely "postulates", only pushes the question a step further back. The digestion of proteins, for instance, is explained in a primary way as due to the action of pepsin and trypsin. But these reagents are elaborated in the cells of certain glands and actually employed outside these glands for the benefit not of the glands themselves but of the body as a whole. Why? And why, again, do zymogens appear just as they do?

Catalysis, Sir F. Gowland Hopkins states, is due to the "highly specialised molecular structure" of colloid surfaces; "were it not equipped with catalysts every living unit would be a static system." Emil Fischer recognised that enzymes distinguished even between optical isomers, and compared their relation with the molecules they acted on to that of a key with its lock. ". . diverse enzymes may act in succession . . . biological syntheses . . . are promoted by enzymes . . . a chain of specific enzymes can direct a complex synthesis along lines predetermined by the nature of the enzymes themselves", Hopkins continues (*Advancement of Science*, 1933, pp. 7-9).

Of hormones he says, each "is a definite and relatively simple substance with properties that are primarily chemical and in a derivate sense physiological." Knowledge of their number and nature still grows rapidly; but we already know a good deal. The auxines in "'higher plants" are "essentially hormones" (*ibid.*, pp. 11-13).

The reaction of a single enzyme can profitably be studied in chemical fashion. But the whole process of (1) the formation of an enzyme, and (2) its appropriate action at the proper time, in the proper place, on the proper matter for the benefit of the whole organism is simply an argument for

vitalism, especially when the vast number of enzymes, known or postulated, in a single body is considered. Nothing of the sort occurs in inorganic nature This was pointed out by Johnstone in 1913 (*Philosophy of Biology*, p 95) I call it the Biochemical Argument (III(*d*), 2, iii, No. 8).

And, on reviewing the history of biology, it will, I think, be seen that the conceptions of Stahl, even of van Helmont, were not so quaint or ridiculous as they appear at first, but were honest attempts to deal with what the biochemist, intent on an artificially isolated problem, often loses sight of: the connection of these remarkable chemical phenomena with the even more remarkable whole organism in and for which they occur.

The same argument applies to hormones, but even more strongly, for they act indirectly, by their stimulation of organs other than those in which they arise, for the benefit of the whole organism As the late Professor Bayliss remarked, they evoke latent potentialities, and zymogens speed up reactions.

(v) *The Gene Theory and Modern Embryology; Durken*

The Gene theory, described in *The Theory of the Gene*, T. H. Morgan, 1926, *The Causes of Evolution*, J B S. Haldane, 1932, and numerous other books and articles, is too well known to need description here. It forms an important part of neo-Darwinian teaching, in which, since the inheritance of variations produced by environment is denied, the only new inherited variations, except a few due to extra-nuclear "plasmons", are those caused by combinations of existing genes or the appearance of new ones (J. B. S. Haldane, *op. cit*, 37-55; E B, art. *Heredity*; Morgan, *op cit.*, p. 289). In other words, Darwin's original idea of small variations due to the environment or to unknown causes is replaced or modified by that of genes, which, as regards the combination of existing characters, is based on Mendel's work[1]; but the causes governing the occurrence of new genes are unknown, although laboratory treatment with X-rays increases the number of mutations.

[1] But in his address to the British Association in 1936, Dr J. Huxley says, "the notion of Mendelian characters has been entirely dropped".

It is beyond question that heredity is conveyed by the two gametes (in bi-sexual organisms), and that it must be carried mainly by the nuclei, which alone are approximately equal in the amount they contribute to the zygote and embryo. Further, it seems equally certain that the chromosomes are greatly concerned in this process. The neo-Darwinians claim that definite regions in these are associated with the inheritance of definite characters, and so regard genes as definite material entities as well as "factors" in inheritance. They regard these "factors" as the equivalents of the characters found in the adult organism.

But, though there is much evidence for this gene theory, it is not universally accepted by biologists. E W. MacBride criticises the gene theory of heredity very ably, especially with regard to sex-inheritance, and quotes Geoffrey Smith, Tornier, and others to show that the inheritance of various "factors" depends upon external conditions and not upon chromosome composition, or at least not entirely so (*e.g. Introduction to the Study of Heredity*, especially Chs. VI, VII, VIII). His criticism that the spontaneous mutants which arise in *Drosophila* are largely non-viable and "cripples" is important (*op. cit.*, p 217); though this is "explained" by the opposition on the ground that existing species are so well adapted to a complex environment that a chance modern mutation is likely to decrease efficiency.

Dr. MacBride's views on heredity are allied with a strong belief in the "inheritance of acquired characters." But this qualification does not apply to Professor B. Durken, whose criticisms of the usual form of the gene theory seem to me to be absolutely valid.

His *Experimental Analysis of Development* gives a modern view of the study of animal embryology Unlike Loeb, Durken has no *a priori* mechanistic theories, he uses this term for processes which occur as in the inorganic world and "mechanical" for any "causal event, without the nature of the causes at work being defined" (*op. cit.*, p. 25). He follows Driesch in other respects too; germ elements possess a prospective potency as well as a presumptive morphological value; regulation is possible in development and regeneration, for "the fate of germinal regions is not definitely deter-

mined in advance by a rigid mechanism of preformation" (*op. cit.*, pp. 28-9) In fact, the experiments he describes and the inferences drawn therefrom are anti-mechanistic except in certain definite cases (*cf op cit*, pp 217-36), such as the tropisms due to temperature, light, and such like, which Loeb stresses in such one-sided fashion. But the recognition of such individualised activities "must not lead us to conclude that development as a whole is nothing more than the sum of these relatively simple physico-chemical actions" (*op cit*, p. 237) Physical and chemical methods can only yield physical and chemical results, only to be comprehended mechanistically; but biological methods, such as "developmental analysis", give us biological results (*op cit*, p. 33). Throughout, Durken emphasises the importance of the organism as a whole (*cf* III(*d*), 1, below), the whole influences the growth of particular parts. The classical Cell Theory led to over-emphasis of the study of parts; but this is now being corrected by the Theory of the Unity of the Organism (*op. cit*, pp 30-33, 250-51)

Many apparently mechanistic results, due largely to Loeb, are frankly admitted, every egg—surely a very sweeping assertion—possesses the "fundamental property" of development without insemination, and the initiation of cleavage is "a purely physico-chemical process." But there is no complete physico-chemical explanation of cleavage in relation to development (*op cit*, pp 52-7).

Study of "Presumptive Organ Regions" (Ch. III) and "The Potency Problem" (Ch IV) leads to vitalistic conclusions; mosaic and regulative eggs are connected by intermediate forms and their development negatives any formationist theory whether old or new, such as the typical modern gene theory (*op cit*, pp. 112-16).

The importance of "organisers" is admitted; but, nevertheless, "It is the whole which is primary . . . in development" (*op. cit.*, pp. 121, 147, 208-9, 237). Modifications due to environment show that development is not automatic (*op. cit.*, p. 166 *seq.*). Determination cannot be due exclusively to material agents; for development, as I consider he demonstrates conclusively, is truly epigenetic, and there is a typical *facies* and form for the whole organism. He

suggests "energy-relations", a sort of "field" analogous to the "fields" of physics, as a non-material "agency" for determination; but this suggestion is admittedly tentative (*op. cit.*, pp 210-12) On the other hand, he will not admit any sort of entelechy (*op cit.*, p 253), which is interesting as his outlook is in other respects so similar to that of Driesch.

Determination of sex by the "material means" of X and Y chromosomes is partly conceded (*op. cit*, pp. 212-13); but it is sometimes due to non-chromosomal factors (p. 264 *seq*).

Chapter VIII, "The Germ-Cell as Reaction Basis", gives a brilliant summary of the clash between analytical embryology and genetics, and an equally able reconciliation of these two methods of research For such rudiments as exist in the unicellular stage correspond neither in number nor character with the final differentiations, production of new manifoldness occurs (*cf* Driesch, III(*a*), ii, above); there is real origination of elementary factors, then of final factors, and only the last named have direct relations with the tissues and organs finally formed So the modern chromosome theory, as usually understood, is incorrect, like the older "mechanistic" preformation theory to which it "belongs." His emphasis on the numerical impossibility of the existence in the gamete chromosomes of genes or factors for every character in the adult is admirable. Also, the facts of regulation make impossible a direct relation between chromosome "rudiment" and final differentiation. Spemann's "progressive organisation" represents the facts of embryology, and epigenesis permits regulation and regeneration, which preformationism does not.

Durken rightly insists that embryology is an older and more important "science" than genetics, and that where "the views of genetics" clash with the sister science "they must undergo modification" (*op. cit*, p 240; *cf.* Driesch, ***Science and Philosophy of the Organism***, p. 154). But the sub-science of genetics is not wholly erroneous; it is indeed "extraordinarily serviceable" (*op. cit.*, p. 242). Its error is in trying to connect the character of the adult with imaginary and impossible discrete genes in the gamete chromosomes; if such discrete material entities exist, which Durken denies (*e.g.* p. 275), they do not correspond with the almost in-

numerable adult characters but are merely the first of many stages in a veritable epigenesis The chromosomes are important, but are not static; they change qualitatively during development; so do the factors or genes which they carry, so that the final factors in the nuclei of the adult have that connection with its phenotypic characters which Mendelism demands; but this does not apply to concrete or other factors in the chromosomes of the germ. And, anyhow, development is not due solely to chromosomes but to the whole nucleus and "the totality of the germ-cell" (*op. cit*, pp. 273-9).

Thus, study of the "mechanics of development" leads to "an evaluation of the organism other than a purely mechanistic one" (*op. cit*, pp 279-80)

My own position is best represented by Durken, though I claim no expert knowledge of embryology. But whatever be the precise truth about genes, two facts seem clear·

Firstly, the occurrence of new "genes" or mutations, like that of Darwin's variations, is unexplained.

Secondly—a matter which is easily lost sight of by those occupied with details of genetics—the whole process of heredity, not only the inheritance of small, occasional, variations, but, principally, the handing down to offspring of practical identity with their parents in thousands, even millions of respects, physical, instinctive, mental—even, in Man, moral, is as astounding as any of the other marvels which science reveals or confirms This stupendous aggregate of events—for heredity affects time in the life of the organism, not merely place—is reproduced through the physical agency of almost infinitesimal specks of nuclear, occasionally cytoplasmic, material All the kaleidoscopic stages of material growth as well as of a never static physical maturity, are, apparently, handed on through the same tiny means as are complicated instincts, and "innate" mental abilities of all kinds. And in every physical element, besides form, there is colour, function, correlation with other parts, and so on. In these marvellous gametes, or their non-sexual equivalents, we have a compendium of all that is implied in the term "organism"—indeed, of all the successive organisms that occur in the metamorphic stages of what we are pleased to

term "an organism" And the more fully organisms "obey" physical and chemical laws the more amazing is the handing down of such capacities—among others, as I believe—through this apparently wholly inadequate material The miracle of heredity is, of course, most striking in animals with high mental or instinctive powers, and principally in Man.

Increased attention to and knowledge of development, including genetics, thus produce not only sympathetic understanding of the vitalistic outlook of Aristotle, Harvey and their successors but an overwhelming argument for the essential saneness of vitalism to-day For even if—which actually seems impossible—it could ever be shown that many millions of special molecules or tiny groups of molecules existed—necessarily in suitable and rapid succession—in chromosomes so that one or more of such molecule groups represented each of the millions of separate factors found in the changing whole produced from the aforesaid chromosomes and their small cytoplasmic envelope, the occurrence and sequence and co-ordinated reactions of such hypothetical minutenesses would be utterly and completely unlike anything else in the physical world. In other words, the facts of heredity lead to vitalism; this is still more obvious if the whole tendency of development, as expounded by Durken, is accepted, as in my opinion it should be.

I would add that it is to me a surprising thing that so little reference is made in modern books upon development to the amazing phenomena of metamorphosis in insects, in which normal processes analogous to regeneration and restitution can be easily studied; these processes certainly suggest vitalism (*cf. World of Life*, pp. 297-304).

# III(*c*).—RECENT DEVELOPMENTS IN PHYSICAL SCIENCE; AND VARIOUS BACKGROUNDS OF BIOLOGICAL THEORIES

"Our little systems have their day"
TENNYSON (*In Memoriam*).

CHAPTER 1

## MODERN DEVELOPMENTS IN KNOWLEDGE AND THEORY OF THE PHYSICAL SCIENCES, AND CONSEQUENT REACTION AGAINST NINETEENTH-CENTURY MATERIALISM

### (i) *General; planes of biological study*

THE bearing of the development of physics upon biological thought before 1895 has been outlined in I(*b*), 2, i, and II(*b*), ii and iii Much of the enormous progress made in the physical sciences in the last thirty years or so does not affect biology directly But in certain respects it does.

The most general of these is the addition of another, lowest, layer to the various planes or layers upon which living things can be considered (*cf* Woodger, *op cit.*, pp. 310-11, 319, 321). These planes can be diagramatically represented thus, placing them in ascending order of complexity.

6. Mental, psychological . . ...... .. ......
5. Organismal; biological . . .. .. .. . ....
4. Cellular; biological and biochemical . . . . ... .... .. .......
3. Molecular; chemical and physical . . . . . ....................
2. Atomic; chemical . . . ....................
1. Sub-atomic; physical (electrons, protons, etc.) . . . .... .... . ........

Physicists are concerned with the properties of matter on planes 1, 2, and 3 in the diagram, but have learned lately

that plane 6 really comes into all their work Much laboratory work in biology does not rise above the level marked 4; I do not think there would be any advantage here in interpolating a stage 4a for histology—nor a 6a for a spiritual sphere. Psychology is naturally concerned with plane 6, though it sometimes touches other planes such as 5, 4, and 1. Plane 6 is placed where it is partly for convenience; there is an unbroken material sequence from 1 to 5 Biologists are or should be concerned mainly with plane 5. But, difficult though it may be, they should remember that the subject matter of plane 5 is built up of matter which possesses modes of coherence represented by the lower planes, and that all their own observations and thoughts depend upon what plane 6 stands for; while plane 6 is certainly to be included in the subject-matter studied as regards Man and, when possible, animals. Biologists must not overdo "abstraction" in their work. The term "matter" and "material" are of course used with full respect to the fundamentally immaterial meanings given to the entities of plane 1, and so of planes 2-5, by modern physics (*cf.* section iii below)

Certain particular impacts of physical research upon theoretical biology now demand attention.

(ii) *Entropy.*

The apparent universality of the second law of thermodynamics has for many years been a favourite theme for physicists, astronomers, philosophers, even theologians.

Sir Arthur Eddington has emphasised it in *The Nature of the Physical World* (1928; Ch. IV, pp 64, 86):

"Shuffling is the only thing which Nature cannot undo. . . . At present we can see no way in which an attack on the second law of thermodynamics could possibly succeed. . . ."

Sir James Jeans qualifies this absolute statement very meticulously, so that the absoluteness of Eddington's statement remains, is even intensified for anyone with some power of grasping very, very enormous numbers:

". . . it is practically certain that . . . the universe will 'evolve' . . . until it finally reaches a state of maximum entropy. Beyond this it cannot go. . . . Yet, if someone

asserts that this will not happen . . he is entitled to his opinion, either as a speculation or as a pious hope All we can say is that the odds against his dream coming true involve a very high power of $10^{79}$ in his disfavour" (*The New Background of Science*, 1933, p 266).

Thus, in contrast to the uncertainty suggested by other findings of modern physics (see next section) the inevitability of entropy increasing throughout is starkly conspicuous. And, if mechanism is the true and adequate explanation of life phenomena, we should expect that the relentless increase of entropy would rule absolutely here, as it does in inorganic nature But this is just what does not happen Although living beings do not, according to all that we definitely know, create energy *de novo*, they do check entropy Jean says "Inanimate matter obeys the law [of entropy] implicitly, what we describe as life succeeds in evading it in varying degrees In fact it would seem reasonable to define life as being characterised by a capacity for evading this law . it seems able to evade the statistical laws of probability The higher the type of life, the greater its capacity for evasion" (*op cit*, p 276, *cf* Ch VIII generally)

Sir F Gowland Hopkins, P R S , though he controverts the use of the expression "evading the law of entropy", said the same in his presidential address to the British Association

"Life has one attribute that is fundamental Whenever and wherever it appears the steady increase of entropy displayed by all the rest of the Universe is then and there arrested. . . in the downward course of the energy-flow it interposes a barrier and dams up a reservoir which provides potential for its own remarkable activities" (*Advancement of Science*, 1933, p 2).

I cannot improve upon what these pre-eminent scientists have said on this subject, nor is it necessary to try; nor can any mechanistic apologist controvert their authoritative statements. Life—which they do not attempt otherwise to define—possesses a capacity for "evading" that law of increasing entropy which all inanimate nature "obeys implicitly" So, for this cause alone, it is futile to try to treat the science of living things as though it rested on

nothing but the same basis, or bases, as inorganic sciences. It must include an element, and a dominating element too, which is peculiar to itself, that is—vitalistic.

(iii) *Indeterminacy.*

Reference has already been made (III(*b*), 3, i) to Jeans' picture of physics as a "ruin", only capable of reconstruction with "observables", which he represents as "mathematics and mathematical formulae" We have moved far from the nineteenth-century period when "most men of science . . . thinking they were keeping clear of metaphysics, accepted uncritically the model of nature put together by science as ultimate reality" (Whetham, *op cit*, p. 316) Instead of solid concrete atoms, Newtonian gravitation, and so on, there now emerge queer, tenuous, ghostly immaterialities such as the electron, which is a group of wavicles, and new concepts such as quanta and "action" "Something unknown is doing we don't know what" is how Eddington describes the mathematical "facts" with which he and Jeans and others rebuild the "ruins" of physics (*Nature of the Physical World*, p. 291)

We now have, Jeans says, two alternatives in physics, a particle picture, and a wave picture. The former is a materialistic picture for those who like to think of matter existing in space and time, the latter is a determinist picture for those who want to know what will happen next (*Advancement of Science*, 1934, p 6) But, "what remains is . . very different from the full-blooded matter and the forbidding materialism of the Victorian scientist", and the wave picture is always supported by observation when it conflicts with the particle picture or "parable", so the former seems to be very near "the truth about nature" (*ibid*, pp. 15, 9 and 11).

It may be objected that, in the address from which I have quoted, Jeans is scornful of such phrases as "materialism is dead" (*ibid.*, p 15), though Whetham's *History of Science* (p. 470) can hardly be included in "crisp, snappy sentences beloved of scientific journalism" But Jeans says that "the wave picture . . exhibits a complete determinism" However, he partially solves the dilemma by pointing out that

"Nature no longer forms a closed system"; and "part of the compulsion may originate in our own minds" (*ibid*, p. 13) He is not sure "whether the determinism of the wave-picture originates in the underlying reality or not", and believes that the problem of free-will will never be decided But, while the old physics seemed to show that free-will was an illusion, "the new physics tells us it may not be" (*ibid*, p 14) He suggests that a solution may be given by what General Smuts terms "holism" (*cf* III(*b*), I, ii); for a beam of light or shower of electrons is probably a truer concept than the "atomicity" of photons or electrons (*ibid*, p 15). Further than this, unlike Eddington, he will not go But it seems clear that he does not contradict the more definite statements of Eddington and Whetham.

Eddington states "It is a consequence of the advent of the quantum theory that *physics is no longer pledged to a scheme of deterministic law*" This, he points out, follows from the Principle of Indeterminacy, based on the mathematical work of Heisenberg and Schrodinger, which Eddington summarises as "a particle may have position or it may have velocity but it cannot in any exact sense have both." So the future is unpredictable (*The Nature of the Physical World*, pp. 294-5, 220 *seq.*; *cf.* Whetham, *op. cit*, pp. 268, 470).[1]

Einstein hopes that a determinism or "strict causality" similar to that of the Newtonian system may be restored some day, somehow (Eddington, *op cit*, p 294). But Whetham considers that from what we know, as distinct from what some people may hope, fundamental indeterminacy is also "reached by way of the doctrine of relativity. . . Physical reality is reduced to a set of Hamiltonian equations. The old materialism is dead" (*op cit*, p. 470).

Eddington places the laws of nature in three classes: (1) identical laws, (2) statistical laws, (3) transcendental laws. The first are "mathematical identities", which "cannot be regarded as genuine laws of control of the basal material of the world" The "apparent uniformity of nature" is largely "a uniformity of averages", which "might well be compatible with a great degree of lawlessness of the individual." The peculiar element represented by statistical

[1] *Cf.* Dr. G. G Darwin, F.R.S., *Adv. of Sci.*, 1938.

laws does not seem to be found in "any of the current conceptions of the world substratum" "If there are any genuine laws of control of the physical world they must be sought in the transcendental laws" of the atomicity of matter, electricity, and "action." Eddington concludes by quoting, like Jeans (*op cit*, p. 3), Kronecker's view of pure mathematics—"God made the integers, all else is the work of man" (*ibid.*, pp. 244-6). In his final summary he states that "strict causality is abandoned in the material world" and probably permanently (*ibid*, p 332). He and Jeans apply this abandonment of determinism only to the freedom of the mind; but the collapse of "the old materialism" may also be applied to general biology. If the physical basis of determinism or "materialism" is undermined, mechanism has no *a priori* right of rule in biology, and so the vitalistic outlook is justified except for such abstracted phenomena as can be reasonably dealt with by physico-chemical methods (*cf.* Woodger, *ibid.*, p 320).

It is well to recall here the limitations of inductive science, on the exact accuracy of which determinism was based. Actually, though the probability may be enormous, induction can reach probability only, never certainty. In any given case or "law" the degree of probability attained must be judged on its merits Modern experience has confirmed in many examples the theoretical criticisms of induction made by Hume, Keynes, and others (*cf.* III(*b*), 1, 1; and Wolf, *Textbook of Logic*, pp. 305-10).

(iv) *Possible limitations of the conservation of energy.*

This principle has been so well substantiated and is so generally accepted, both by theorists and by engineers and other practical people, that its reliability has been assumed throughout this book; while its establishment has been duly noted as a "blow" to the uncritical vitalism of the preceding period (*cf.* II(*b*), ii).

But it is well to note that there may be limits even to this great principle, which has accorded so gratefully with materialism and "scientific naturalism."

Jeans (*Advancement of Science*, 1934, p. 11) points out that, out of many electrons with a small voltage, a few will sur-

mount an enormously greater adverse potential; which disagrees with the great "law", to which, theoretically, there should be no exceptions. As long ago as 1904 Whetham challenged the then almost universally accepted laws of conservation of matter and energy "Momentum and entropy," he wrote, "are only conserved under restricted conditions"; it was "dangerous to assign too much philosophic importance to these principles of persistence" (*Recent Developments of Physical Science*, 5th edn, 1924, p. 39).

The work of Atwater and Benedict, T B Wood, and others showed that the human body obeyed the principle of conservation of energy; "if, as is probable, intellectual work or other uncounted activities use up energy, the amount must be small," Whetham concludes (*History of Science*, pp. 279-80). But even this small probability must not be lost sight of. The discovery of the inert gases was due to Lord Rayleigh's attention to minute differences in measurements.

CHAPTER 2

# VARIOUS BACKGROUNDS OF BIOLOGICAL THEORIES

(1) *National characteristics or backgrounds of thought.*

THE characteristics which will be briefly considered here are those of certain European nations; several admirable smaller ones among them must be omitted for lack of space. Although modern original work in various branches of science is being done by some Indians and Japanese who follow "Western" methods, the history of science is actually a history of the thought and experiment of some of the nations of Europe, with relatively small additions from their descendants in the British Dominions and the United States. The vastly greater populations in other continents have contributed little or nothing. This historical fact has been noted by Ball (*History of Mathematics*, pp 8-10, 162-3), Fleming (*Origin of Mankind*, pp. 134-5), Merz (*History of European Thought*, vol i), and Whetham, who says.

"Philosophers have declared that men are born equal. The naturalist knows that statement to be untrue. . . . The unequal distribution of illustrious men among the nations is a biological fact. France, Great Britain, Italy, Germany, and some smaller groups, have since the revival of learning contributed many such men .. The inferiority of the proletariat nations becomes still more manifest" (*op. cit.*, p. 358).

This is not contradicted by the truth so exquisitely phrased by the poet Gray. Flowers and gems, known or unknown, occur in suitable habitats.

The significance of national modes of thought and outlook with regard to scientific problems, especially those connected with philosophy, is also well brought out by Whetham in his admirable *History of Science and its Relations with Philosophy and Religion* (2nd edn , 1930).

J. Needham has observed that "certain types of research flourish especially in certain countries at certain times", he quotes Germany for amphibian and experimental embryology, the U S.A. for invertebrate and comparative embryology, and now Russia and Poland for "causal morphology" (*Science Progress*, July, 1936, p 52) But wider differences than these exist

As in Russia in 1917, the revolution in France in 1793 was largely hostile to intellectual leaders But the new French rulers soon realised their need of science, and "in the early years of the nineteenth century the scientific centre of the world was Paris" The Academy was reopened, with Cuvier as its permanent secretary under Napoleon, and the close connection between science and literature that had been the glory of France in the eighteenth century was regained (Whetham, pp. 309-10, *cf* Merz, i)

Cuvier's powerful influence was a double one He promoted the progress of science in general and palaeontology in particular. Also, he opposed the loose ideas of the period about evolution so successfully that for long after his death Frenchmen were slow to adopt transformism, especially Darwin's exposition thereof (*cf. From the Greeks to Darwin*, pp 195-6, 202-4) On the other hand, according to McDougall, modern French biologists are exceptionally sympathetic to the evolutionary ideas of the Frenchman Lamarck (*Biology and the Sciences of Life*, p 179). French biology has a national tinge.

German thought in the early decades of the nineteenth century was largely dominated by nature-philosophy Despite some able discoveries by Goethe and others, this was mainly a mass of deduction from fanciful theories. But from about 1830 regular scientific procedure was followed in Germany, and for several decades the progress of botany was almost entirely in the hands of Germans, such as Nageli, von Mohl, and Hofmeister (Sachs, *History of Botany*, Part I). Then from 1860 or so German biology was again dominated by the excessive cult of a philosophic idea, this time *Darwinismus*, despite the opposition of von Hartmann, Nageli, Eimer, von Baer and others. This is also illustrated by the whole history of German *kultur* and politics from about 1860

till the Great War, as V. Kellogg has shown (Woodger, *op cit*, pp. 474-5; *cf.* H S. Chamberlain's *Foundations of the Nineteenth Century*; Whetham, *ibid*, pp. 330, 341; and histories *passim*) But the modern reaction towards vitalism has been largely the result of the brilliant experimental work of the German Driesch (*cf* Part III(*a*)), and is seen in other Germans such as Rádl and Durken

Russia has contributed several illustrious names to the history of science, though but few in proportion to its population if judged as a European country Its scientific work since the Revolution of 1917, though intensified in certain directions with a materialistic bias, requires critical examination, especially as regards biology and freedom of thought

*Soviet Science*, by J G Crowther (1936), seems to promise suitable information, though it "contains little discussion on the relation of political freedom to scientific research and discovery" (*op cit*, p ix) Its first chapter praises Lenin as a writer on "the philosophy of physics", and stresses the close connection between "soviet science" and "dialectical materialism", which means the philosophy of the Jew Karl Marx, as amplified by F Engels, Lenin and other Bolsheviks; "Part VI" also refers This book is mainly occupied with physical science and its practical applications Part V deals with biology, and gives an interesting outline of the work and aspirations of Vavilov and his assistants

The high-sounding, mysterious "dialectical materialism" upon which Crowther's lauded soviet science is built, turns out to be nothing but Marx's very crude and naive materialism, dating from the 1850's (Cole, *What Marx Really Meant*, p. 287 *seq*), *plus* an adaptation of Hegel's idea of change (*ibid.*, p 284 *seq*). to suit Marx's obsession of a class struggle lasting till his "proletariat history" begins (*ibid*, p. 289 *seq*). As Dean Matthews says, it is the only philosophy which has included a contradiction in its very title (*Modern Churchman*, September, 1936, p. 234).

How far *Soviet Science* really represents the general and scientific position in Russia, especially as regards biology, is shown by study of many authors of various nationalities, some formerly adherents of Bolshevism.

Karlgren (*Bolshevist Russia*, 1927) exposed the tyranny of the new rulers (Ch I), and the falsity of soviet claims made for cultural progress (Ch VIII), "natural science is allowed only if it starts from undiluted Darwinism" and is useful to Communist society, and non-Communist scientists are oppressed and ill-treated (*ibid.*, pp 295-304) This was confirmed by Hindus (*Humanity Uprooted*, 1929, Ch. XIV), the life of the *spetz*, the professional man, is very circumscribed, and the clergy do not share even their paltry privileges Recent confirmation of the wholesale atrocities, oppressions, and falsehoods of the disciples of "dialectical materialism" are given by Chamberlin (*Russia's Iron Age*, 1935, Ch. VIII), Lazarevski (*Under the Bolshevik Uniform*, 1936, Ch VI), and Panait Istrati (*Russia Unveiled*, 1931). Chamberlin notes that elementary schools have improved, but emphasises the obliteration of universities (Ch XV); and describes the virulent official war against religion in the schools and outside, and the attempts to conceal it from outsiders (Ch. XVI) Lazarevski stresses the soviets' implacable hatred of religion, their murders of thousands of clergy, and perversions of education (Ch III), while Istrati dwells on the stupidity of their dupes in other countries.

To sum up· the leaders of Russia's dominant oligarchy have been personally prominent in the "anti-God" campaign. Lunatcarsky, late Commissar for Education, expressed such sentiments as, "We hate Christians . . . Down with love of one's neighbour" (*Action*, 3rd September 1936). There remains a strong Communist influence against religion and for atheism and materialism, and consequently against the non-deterministic and vitalistic movements typical of modern science and philosophy in such countries as Great Britain where thought and its expression are free (*cf.* III(*b*), III(*c*), 1, and III(*d*). So biologists in Russia are subject to strong mechanistic influences, and their expression of contrary ideas is difficult and even dangerous.[1] Only in

[1] The reviewer of Golomshtok's *Old Stone Age in European Russia* frequently refers to the effect on the palaeontological views of Russian scientists of the communist "materialistic interpretation of history" and "the evolutionary materialistic explanation of cultural changes" (*Nature*, 15th Oct 1938, 693 *seq*). *Cf* correspondence, *The Author*, Spring number, 1939.

1936 Berdyaev, the historian, had to leave Russia for this reason.

The interactions of Roman Catholicism with science are complex, and are often presented from a biased point of view. Roman Catholic apologists represent that Church as conserving knowledge during the barbarous Dark Ages and in later times welcoming scientific truth as against some scientists' impious and inaccurate theories. The "rationalist" school dwells upon the horrors of the Inquisition and the undoubted political tyranny which Rome has employed whenever possible to crush religious and scientific views other than its own, and naturally finds in the "Index" a bar to freedom of thought (*e g* Bury, *History of Freedom of Thought*—a very prejudiced book).

The Roman Catholic record of persecution and political intrigue is a scandalous one; but the interactions of Romanism with science are not properly represented by either extremist view. Individuals, including Descartes, Desjardins, Mivart, and Pasteur, have combined biological research with genuine belief in Roman Catholic doctrine. A fair presentation of the position as regards organic evolution is given by Osborn in *From the Greeks to Darwin*, Ch. III.

Yet the fact remains that, ever since the hardening of Roman Catholic teaching in the sixteenth century, the position of scientific thinkers in countries dominated by the Roman Catholic Church has been one of great difficulty; the usual alternative in such countries as France and Spain has been an agnostic or even atheistic materialism which challenged or denied any spiritual interpretation of the cosmos. This clash is peculiarly severe as regards biology, taken in a wide sense, and Sir B. Windle's clever effort to reconcile scholasticism with evolution has had few followers. Mivart was excommunicated. However, it would be a serious mistake to regard all Roman Catholics as pledged to a narrow "fundamentalist" position; at the present day their astronomers take an honoured part in the discussion of cosmic problems, and such priests as Schmidt and Schebesta deal with anthropology with apparently no greater inhibitions than those imposed on other researchers by an *a priori* mechanist philosophy.

No one has summarised the British outlook on science and the allied problems of philosophy and religion, its essential reasonableness and its, to many continental observers, puzzling lack of consistency, better than Whetham Though Newton was essentially a son of Cambridge, he does not invalidate Whetham's claim that "perhaps the most striking peculiarity of English science has been its individualist spirit, and the frequency with which work of brilliant genius has been done by those of no academic position" (*op. cit.*, p. 310, *cf* Merz, *op. cit*, 1, III)

Whetham gives many illustrations for his thesis Empirical psychology came naturally to the "common-sense" outlook of Englishmen and Scotsmen who were able to follow a line of thought "as far as it proved practically useful, without reference to its apparent logical effect on other subjects. . . . In physiology and experimental psychology it is necessary to suppose that animals are subject to . . physical and chemical principles, and that man is a machine. . . But when continental pseudo-logicians argued that this useful working assumption represented reality, and that man is *nothing but* a machine, the British . saw that though it was in accordance with one set of facts, it did not agree with another, and they were quite content to regard man as a machine in the physiological laboratory, as a being possessing free will and responsibility when they met him in the ordinary affairs of life, and as an immortal soul when they went to church. . . . This characteristic British attitude of mind . . . though it seems illogical to continental minds . . . may still be the true scientific attitude. . . . At the present time (1929) physics . . . is using two fundamental theories apparently quite inconsistent with each other, thus perhaps justifying the British habit of mind" (Whetham, *op. cit*, pp. 324-5).

This passage, with which I heartily agree in the main, can be substantiated from non-British authors of unquestioned eminence. For instance, Driesch, formerly President of the London Society for Psychical Research, wrote with reference to this debatable subject, "A model for the conduct of such polemics is provided by the English writers" (*Psychic Research*, p. 4).

Needless to say, it is not claimed that all British scientists possess these admirable qualities, or that they are unknown outside the British Isles; and their continuance is threatened by a dysgenic birth-rate (*cf.* R. A. Fisher, Whetham, Dean Inge, etc.).

(ii) *Personal beliefs of biologists, philosophical, religious, or anti-religious*

Against national, popular, or ecclesiastical ideas individuals have been in constant conflict. Sometimes they were successful during their lifetime; sometimes recognition and triumph were posthumous. But we must guard against the insidious fallacy that the individual thinker is usually right and popular belief wrong. For every one man who figures as a hero in history, scientific or general, because he corrected contemporary error, there are scores who have rightly sunk into oblivion, victims of some fixed idea who failed to appreciate what the majority had accepted on solid grounds as correct. Between these two classes—the few in advance of their period and the many behind it—come those who achieved success for ideas which later were found to be false. Examples of all these types abound in the history of science.

Here we consider the effects of non-scientific beliefs of biologists upon their attitude towards biological theories. Such beliefs may be due to many causes. In Part II(*a*) the general belief of Christians in the seventeenth and eighteenth centuries in a literal interpretation of the first chapters of Genesis has been discussed as leading not only to erroneous views about geology but to a non-critical acceptance of vitalism. At that period this was a European background of thought, which is not now the case. But while scientists can have no sympathy with the legal prohibition of evolutionary teaching in Tennessee, U.S.A., there are some well qualified medical and scientific men in Britain who, for historical, personal, and archaeological reasons, accept the teaching of the Bible as true in all important respects and so argue that its earliest records are likely to be substantially true too. This view has lately been ably expressed by Sir Ambrose Fleming, F.R.S., in *The Origin of Mankind* (1935). The net result is that, as regards biology, a striking resemblance is

reached to the views of Professor Berg, although the latter omits reference to a Creator (*cf.* III(*b*), 3, iii).

Driesch (III(*a*)), Wallace (II(*d*), ii), and McDougall (III, 2, iii), are examples of eminent scientists whose outlook, in their early days profoundly materialistic, has changed to one which can be broadly described as vitalistic in biology and religious in general Belief in vitalism then may be forced on a mechanist by study of biology, as illustrated in the arguments of Driesch and Durken; but it comes more easily to those who are already influenced by ideas outside the orthodox boundaries of science, whether religious, as for Sir A. Fleming and his friends, or psychological, as for McDougall and Wallace.

Either physics and chemistry explain everything, as in Haeckel's monistic "philosophy", or they do not To those who believe in peculiar spiritual and mental capabilities in Man and in the need for a Creator of at least the beginnings of energy and/or matter and of life—whatever the exact mode of creation may have been (*cf* Fleming, *op cit*, pp. 21-2), it is natural that life phenomena which do not agree with inorganic "laws" should be of a separate, vital or vitalistic order, just as in chemistry we deal with arrangements different to those concerned in physical changes (*cf* III(*d*), 2).

Scientists such as Newton, Faraday, Kelvin and others already mentioned, pursue knowledge with the usual methods as far as possible, with, of course, the underlying assumption that the universe is rational and orderly on the whole (*cf.* A Wolf, ***Textbook of Logic***, Ch. XXIII) But if they seem to come up against a dead end, impenetrable by orthodox, restricted scientific methods, they admit the possibility of other factors and do not try to force a gallon into a pint pot.

On the other hand, biologists are, after all, simply human beings like other men and women. To some of them the traditional arguments for the existence of God seem weak and unsound; for various reasons religion appears to have nothing that has not been put in by Man himself; they have no personal knowledge of that validity of religious experience of which, Whetham considers (*op. cit.*, p. 485; *cf* III(*d*), 2, iv, No 13) science must take account. Over a wide field, even

a widening field, mechanistic methods operate successfully and in a manner which appeals strongly to the mechanically minded men who—all honour to them—play such a large part in the development of science and in the necessary destruction of much mental rubbish and superstition which have clogged progress in the past in science, religion, philosophy and practical human affairs.

Often, as for laboratory workers whose studies are restricted to interesting but limited fields, biology presents itself as a branch of physics and chemistry; and so all hangs together and suggestions for other considerations may appear unnecessary or fallacious

There is an illuminating passage in Haeckel's *Wonders of Life*, p 443 As a lad young Ernst was given materialistic teaching, akin to transubstantiation, about the Lord's Supper by a Lutheran minister, whom he admired greatly. He formed the impression that abnormal physical sensations would be perceptible, which, of course, he did not experience. Though his parents managed to assuage his "want of faith" at the time it is highly probable that his adult atheism and materialism were due to the erroneous religious instruction he received in his early years. A R Wallace experienced something of the same nature (*cf* *My Life*, ii, 349).

So individual biologists depart from the current views of their predecessors and contemporaries Haeckel, Schwann, and other nineteenth-century naturalists who began as pupils of J Muller headed the movement away from the vitalism of their master; in a later generation, in the same country, Driesch and Rádl similarly turned against the mechanistic monism of Haeckel

In palaeontology we have a mixture of biology and physical geology, anatomical knowledge of a fossil is of little importance unless the formation in which it occurred is also known, as various recent controversies about human remains have shown. The influence of palaeontology upon the evolutionary ideas of biologists has varied greatly. Darwin was profoundly affected by the uniformitarian ideas which Lyell developed after Hutton; and so are many neo-Darwinians, though nowadays uniformitarianism is less rigorously held than in Darwin's day. On the other hand, Berg,

Broom, Tansley, Scott and other students of palaeontology find its evidence for "Darwinism" unconvincing, and draw conclusions favourable to a more definitely vitalistic outlook

Passing reference may be made here to the important effect of travel, not of mere transit but of prolonged residence in various parts of the world, upon the ideas of individual biologists. Linnaeus travelled extensively in Northern Europe The overseas researches of Darwin, Wallace, Hooker, T H. Huxley and others had a great effect on evoking not only the biological application of Malthus' idea of natural selection from Man to all plants and animals but a philosophic view of the complexity of the species problem in space as well as in time

An allied problem is really philosophical, but it sometimes affects people's views of biological theory—the problem of pain. If, as has often been movingly urged, animals have for enormous ages experienced dreadful suffering, if they are haunted by fears of death and pain for themselves or others such as human beings can experience, this is a philosophic argument for mechanism; sentient beings are the miserable prey of blind forces of nature. This conception has often been put forward by people with more sentimentality than sense; in its extreme form it is anthropomorphism verging upon animism, for human powers of thought and feeling are attributed to animals with very elementary nervous systems. Yet its propagandists can claim at least one great biologist on their side—T. H Huxley (*Nineteenth Century*, February 1888, pp. 162-3).

The fundamental inaccuracy of such opinions has been admirably exposed by Wallace, who quotes Darwin to good effect (*World of Life*, Ch XIX). Yet on this subject both write as orthodox Darwinians The explanation of this diversity seems to me to lie in personal, non-scientific experience. T. H. Huxley's health was poor and so were his finances when he was young. Wallace possessed remarkable vitality and also was able to take a consolatory view of his own pecuniary difficulties owing to his eventual belief in man's nature as essentially spiritual (*My Life*, i, 15 and Ch. XXXIX) Darwin was always well-to-do, and as a young

man was physically robust Huxley was led by his non-scientific beliefs to project into the animal kingdom a great deal of misery which, as Wallace has shown, does not actually exist Pain and discomfort there are undoubtedly, but not more than is necessary to promote safety and, probably, progress; while the capacity for enjoyment proceeds *pari passu* with the capacity for pain

This view is, firstly, in accord with the facts, which is what matters most Secondly, it weakens or even destroys a metaphysical argument used to buttress mechanism Thirdly, in my opinion, it agrees with the modern vitalism described in Part III(*d*) below.

(iii) *Conclusion Vitalistic or mechanistic judgements in Biology are largely determined by non-scientific beliefs or prejudices*

The history of biology, as of medicine—like the general history of mankind, according to Carlyle—is largely that of great men. Harvey, C F Wolff, Jenner, Pasteur, Lister, Darwin, Wallace and others got ideas in advance of their fellows, and sooner or later these ideas became generally accepted, though their exponents were often met with criticism and opposition, even in the scientific circles of their time, notably from the ultra-conservative medical profession; others, like Camerarius and Mendel, suffered neglect and contempt

But the average biologist, however intelligent and single-minded he be, is likely to be affected by the background of thought of his period, particularly as displayed in his own nation. Sometimes this background is dominated by a scientific theory, *e g Darwinismus*, sometimes by religious or anti-religious bias; partly it is due to national modes of thought, which may be too narrowly logical or insufficiently critical, or tainted with political or racial feeling There is always a hard scientific orthodoxy, which opposes research outside certain limits which are supposed to contain all the subject matter of science. In biology, as distinct from physical sciences, it is difficult for the thinker to be unaffected by his general metaphysical ideas and his general knowledge and ignorance; for biology must involve his ideas about Man Like J. Needham he may make an artificial boundary to

suit his convenience or taste; but this distinction cannot be maintained when biology comes into contact with palaeontology, anthropology and so on. Dr Needham may be able to join in the liturgy one day and study mechanistic biochemistry on another; but his brother physiologists will enquire curiously whether a taste for religion is due to the hormonic influence of some peculiar chemical in Dr Needham's make-up or to some other factor which his limited biology should take account of but cannot

So judgements as to whether the whole of the subject-matter of the science of life is explicable by mechanism or whether it involves other elements too may be partly based on biological data, such as those derived from study of development (*cf* III(*a*) and III(*b*), 3, v); but partly they will be determined for each thinker by his non-biological beliefs, prejudices and experiences. Woodger has emphasised the trouble caused in biology by "antitheses resulting from subjective influences" (*ibid*, p 269) So biology should be correlated as widely as possible with all branches of knowledge and experience, whether deemed scientific at any particular period by the official exponents of "science" or not Otherwise biology will not really be biology at all.

# III(*d*) —CONCLUSION

"However numerous our observations may be, yet if they only bear on one side of a question, they will never enable us to penetrate the essence of a natural phenomenon in its full significance "
LIEBIG (*Animal Chemistry*, 1842 ; Preface, xviii.)

CHAPTER 1

## REVIVAL OF CONCEPT OF THE ORGANISM, AND OF THE VALIDITY OF "NATURAL HISTORY"

### (1) *General, and Johnstone's* Philosophy of Biology

THE modern revival of the concept of the organism as a unity, as stressed by Durken (*cf* III(*b*), 3, v), is connected with the anti-mechanistic biology of Driesch and the hormic ideas of McDougall and others But it reaches back to that fundamental study of living creatures in their environment which was maintained by Fabre and Wallace (*cf* II(*d*)), used by Darwin, Belt, Bates and F Muller, by R Brown, Hooker and other botanists, by Linnaeus, Belon, Rondelet, Gesner, by old Aristotle and some writers of the Old Testament, and even by the ancient artists who painted faithful studies of beasts, birds and fishes amid conventional human figures in the great queen's temple at Deir el Bahri, Egypt, and on the walls of Babylon and other mighty cities of the past.

The need for consideration of "the whole organism" in physiology was well maintained by J S Haldane, the vitalistic import of his ideas being stressed by Singer, the historian of medicine and biology (*History of Biology*, pp. 406-8).

James Johnstone's *Philosophy of Biology*, written in 1913, occupies an interesting half-way position between early twentieth-century vitalism and its subsequent developments. Though he pays tribute to the importance of physico-

chemical methods in physiology he shows clearly that these methods are insufficient to account for the activities of the whole organism (*op cit*, pp 109, 113, 273) The vitalistic import of animal embryology is enforced by apt quotations from T. H. Morgan, "anything but a vitalist" (*ibid*, p. 143), and Loeb's theories are again well criticised (*ibid.*, pp 146-7, 181 *seq*)

Many reasons for rejecting mechanism and adopting vitalism are given, though not always clearly (*e g. ibid*, pp. 278-9); but the entropy argument is well used, with extension to the movements of dominant animal groups (*ibid*, pp. 310-18); and the "formal description of the organism" gives an interesting summary of many vitalistic arguments (*ibid.*, pp. 331-7).

In *The Mechanism of Life* (1921) Johnstone gives in simple language a good historical account of energy and its transformations and of the mechanics of the animal body (*op cit*, Chs. I-VII), though always with the whole life of the organism kept in view in a way often lacking in textbooks on physiology In Chapter X science is defined in a mechanistic way; but he shows that in animals there is an ascending scale of spontaneousness or consciousness, traceable from plant and other tropisms, through invertebrates, lower vertebrates, and "spinal" animals, up to what we find in unmutilated higher vertebrates and in Man (*ibid*, Ch VIII). So animal behaviour is increasingly spontaneous, and conflicts with the determinism found in inorganic nature and in the simple reflexes of lowly organisms (*ibid*, pp. 171, 174). "Indeterminism of response" is developed somewhere in the ascending scale of living things; though its occurrence in "the typical plant organism" is dubious (*ibid*, p 139).

He supports Driesch in his criticism of the conservation of energy as an "*a priori*" law of our mentality rather than a physical one (*ibid*, p. 194). But he here objects to Driesch's entelechy and other neo-vitalistic "agencies that cannot be investigated" (*ibid.*, pp. 193, 215); yet he then argues for a "life intuition", which he cannot define; though it is "non-energetic, non-measurable, *and is not conserved*" (*ibid.*, pp. 223-4). So Driesch's experimental proofs for vitalism are

ignored, while the most negative and unsatisfactory features of his entelechy doctrine are retained!

Johnstone's ideas of infinite time and possible reversal of entropy are out of date (*cf* III(*d*), 2, ii) and lead nowhere as regards his biological quest, yet "we have . . . to find some concept which will be special to the phenomena we call vital ones", and this "vital" concept he now finds in the retardation of the increase of entropy (*cf* III(*c*), 1, ii).

This book is as unfavourable to Driesch as his first was favourable. But he still quotes Bergson, though less wholeheartedly.

(ii) *The present position, Hopkins and the concept of "Organisation", J. Gray and the "living organism"; E S. Russell, animal behaviour and natural history*

The modern return to the "whole" or organism concept is excellently illustrated in the presidential addresses delivered to the British Association and its Zoology Section in recent years.

In 1931, its centenary year, the presidential address was given by General Smuts Whatever may be thought of his particular theory of wholes, his selection as president of this important meeting is significant, for he represents an anti-mechanical view of things in general which is typical of most modern scientific thought. In his "scientific world picture" life and mind are stressed as independent, with an autonomy subject only to a general extension to all phenomena of the concept of organism. This is not vitalism (*cf.* III(*b*), 1, ii); but recognition of a considerable autonomy for life and mind comes close to that philosophical theory termed "emergent vitalism" (III(*b*), 1, iii) and is anti-mechanistic.

The views now to be considered are those of experts in various branches of biology

Sir F. Gowland Hopkins, P.R.S., in his address on "Some Chemical Aspects of Life", after referring to "the arrest of energy degradation in living Nature" as "a primary biological concept" (*cf* III(*c*), 1, ii), said, "Related to it, and of equal importance, is the concept of Organisation" (*Advancement of Science*, 1933, p. 2). His account of biochemical research, he rightly claimed, did not raise the old merchanist-

vitalist controversy; but his findings are of considerable interest in this connection.

Thus, his claim that biochemistry must "contribute to an adequate description of life is not to imply that a living organism is no more than a physico-chemical system" (*ibid.*, p. 4) He refers appreciatively to the "holistic" Centenary Address of General Smuts and to a subsequent one in which congruence is admitted between biochemistry and the view that "a part in a whole is not the same as the part in isolation" (*ibid*, p 16). And, though he believes that chemical organisation alone accounts for the ability of every living cell "to maintain a dynamic individuality in diverse environments", "living cells display other attributes even more characteristic of themselves", "they grow, multiply, inherit qualities and transmit them", so, "it is not illogical to believe that such attributes are based upon organisation at a level in some sense higher than the chemical level" (*cf.* III(*c*), 1, i, above) But to distinguish such levels may be to abstract from reality, and, following Occam's dictum, we should be sure of the limitation of chemical agencies before seeking for "super-chemical organisms" For instance, Spemann's "organiser" may be "a hormone in essence", widely distributed outside the embryo (*ibid*, pp 16-17).

So Sir Gowland rightly stands for the interpretation of as much as is possible in organic life in terms of the simplest agents. But this review by the greatest living biochemist is, despite his disclaimer of indifference, distinctly favourable to vitalism. He concludes as follows:

"If the biochemist should at any time be inclined to overrate the value of his contributions to biology, or to underrate the magnitude of problems outside his province, he will do well sometimes to leave the laboratory for the field, or to seek even in the museum a reminder of the infinity of adaptations of which life is capable."

And to Lord B. Russell's typically pessimistic estimate of life as "from a cosmic point of view . . . a very unimportant affair" Sir Gowland replies that "it may be small in extent, but is the seat of all the most significant events" (*ibid.*, pp. 17, 18).

The addresses to the Zoology Sections of the British

Association in 1933 and 1934 both demonstrate in modern language the utter inadequacy of mechanistic views for biology.

In 1933 Dr J Gray, F R S, discussed "The Mechanical View of Life" He recognises with Haeckel the importance of the origin of life in this connection, but here we only note that he finds in biogenesis an argument for the autonomy of life (*cf* II(*c*), iii) and pass on to his other reasons for regarding the organism as an independent unit and biology an independent science The whole paper deserves careful study An organised dynamic structure is the central characteristic of living matter, and, being unknown in comparable inanimate systems, must be regarded as a fundamental attribute of living matter, alternatively, if the fundamental unit of life is extremely small, statistical laws of physics seem as inapplicable here as they are to atoms of radium (*Advancement of Science*, 1933, pp 86-8) Arguments from modern experimental embryology, impossible to condense, indicate that "the cell has an individuality of its own" and that the organism has a degree of complexity unknown in the physical world, is dynamic and essentially unstable, yet builds up instead of breaking down (*ibid*, pp. 90-1).

"(1) An inherent complexity of structure and (2) a dynamic potentiality of initiating events" never or rarely found in inanimate systems are inferences derived most clearly from embryology, but also from "another great sphere of experimental enquiry . a study of the relationship between the fully grown organism and its physical and chemical environment", provided that attention is not concentrated on the physical events only (*ibid*, p 91)

His summary is that: (i) the laws of physics are—at least at present—statistical only, they express probability and not absolute truth, so, if accepted for biology, their conventions must be accepted too, and excessively improbable events, such as abiogenesis, must be assumed not to have occurred; (ii) however, the development of an organism involves processes without parallel in inanimate nature, so "it is dangerous to assume that the statistical laws of physics can satisfactorily describe all biological events"; (iii) the fundamental unit of living matter is probably "extremely

small" and contains "potentialities for change which are unique in the universe" These must be accepted as "fundamental axioms" for biology As Niels Bohr says, the existence of life is an elementary inexplicable fact, which "must be taken as a starting-point in biology", like the quantum of action which is irrational according to classical mechanical physics, though forming, with the existence of elementary particles, the foundation of atomic physics; (iv) the zoologist, unlike the physiologist, cannot restrict his interest to "the physical properties of isolated organs", but must remember the "'intrinsic potentiality of the living organism'" For the first of these studies all the resources of physics and chemistry must be used; for the second "biology must be the mistress—not the servant—must make her own foundations and build on them fearlessly, prepared to change her views, if need be, but not prepared to force the wine of life into bottles designed for use in the simpler . . . fields of chemical science."

Dr E. S. Russell, addressing the Zoological Section in 1934 on "The Study of Behaviour", expressed entire agreement with Dr Gray's view that the organism possesses "properties and potentialities" not reducible to those shown at the chemical level, and that experimental biology should be more concerned with the organism as "a single living unity"

He then dealt with animal behaviour as a highly important "whole-activity of the organism." The zoologist must study behaviour "in the field"; animal ecology or bionomics is essential for the interpretation of laboratory experiments (*Advancement of Science*, 1934, p. 83).

Despite much excellent work in scientific natural history, the study of animal behaviour has been much neglected in Britain; it has been treated as a laboratory subject, subordinate to physiology or psychology. And the fault is largely due to biology following belatedly the mind-body dualism of Descartes Biology still suffers from "the classical doctrine of materialism", which other branches of science and philosophy have outgrown (*ibid.*, pp. 84-8). Russell, with Koffka and others, rejects the conditioned reflex explanation of Pavlov as (1) based on pure hypothesis,

derived from Cartesian metaphysics, and (ii) not harmonising with "the simplest facts of observation" (*ibid*, p 95). With this fundamental criticism I agree entirely; it also applies to much of Loeb's work E S Russell's powerful criticisms of Descartes and Pavlov are summarised in Parts I(*b*), 1, i, and III(*b*), 3, ii Here we are concerned with his positive statements.

He follows Whitehead in regarding matter and mind as abstractions, and turns to individual experience which involves "only the perceiving subject and his objective world", which, of course, includes smell, colour, and all the qualities which philosophic "materialists" abstract from it. Zoologists must study animals in action, and accept "the obvious facts at their face value" (*ibid*, pp 86-8)

Thus, "living matter" has no existence except in actual organisms, and each living thing passes through a cycle of activity, normally comprising development, reproduction and senescence leading to death. Time is involved in the definition of an organism too—a "'dynamic pattern in time'" (Coghill) Russell borrows from C S. Myers (*The Absurdity of any Mind-Body Relation*, 1932) the word "directive" to describe the quality of vital processes, whether conscious as in ourselves or as found in developing embryos. The idea of purposiveness is immaterial "from our simple objective point of view" Behaviour, he maintains, is "simply one form of the general directive activity of the organism", and is seen better in animals than in plants, for typical animals can respond to environmental changes and satisfy their needs by movement, but plants show it equally well in the "general sense" (*ibid.*, pp. 88-9)

According to E S. Russell, the modern concept of "organism" avoids both mechanism and vitalism (*cf* Hopkins above), as it ignores the concepts of matter and mind. But though molecules and atoms are "organised systems", he does not agree with those who see no essential difference between organic and inorganic "unities" (*cf.* III(*b*), 1, ii, Whitehead; and iii, Ll. Morgan). Though all are systems or "wholes", the living organism possesses characteristics "which are lacking in inorganic systems", for instance "the weaving together in one cyclical process of the master functions of

maintenance, development and reproduction," and "their constant drive towards a normal and specific end" This view, he says, makes no real distinction between life and mind, between vital and mental or psychical activities (*ibid*, p. 90).

Physiological analysis is useful in its own way, but "the study of the whole, as in behaviour, cannot be adequately replaced by the study of the parts in isolation . . . A good deal of what ranks nowadays as experimental biology is not biology at all, but physico-chemical research carried out on organic systems" (*ibid*, p. 92) "Straight natural history in the old sense", the study of the "habits" of animals, linked with modern ecology, is the basis for detailed study of behaviour, and the clue to much observed under artificial, laboratory conditions.

The address is illustrated by accounts of actual studies. Thus, a caddis larva, if disturbed in various ways, uses different methods to restore its normal relationship to its environment "The end is more constant than the method of reaching it" (*ibid*, p 93, *cf* Driesch, p 115). But often, as in birds' nesting arrangements, a directive activity complete in itself is also only one link in "the long reproductive cycle"; "the whole life-cycle must be regarded as the primary thing", and we must guard against the "inveterate tendency" of the human mind to separate elements of fundamentally continuous processes. Events must be related to what follows, not only to what has gone before (*ibid.*, p. 94).

How is behaviour initiated? A large part of animal behaviour is response to needs, or deviations from the normal; it is influenced by events in the environment which the animal perceives and responds to, such as signs of danger or of food As regards perceptions, especially visual, by animals, "the whole trend of modern work . . . is to show that they do not normally respond to simple physico-chemical stimuli, but to more or less complex whole-situations." This is illustrated by references to Bierens de Haan, Kohler, Kluver, and the Gestalt psychologists (*ibid.*, pp 95-7). My own observations in the field, mostly tropical, agree (*cf.* p 85 above).

Russell sums up under two heads. Biology must free itself

from "the limitations imposed upon it by a blind trust in the classical doctrine of materialism"; and a more concrete and adequate concept of the living organism as a "four-dimensional whole or directive cyclical process" is necessary (*ibid.*, p 98; *cf.* Gray above).

Though he repeats (*ibid*, pp 90 and 98) that this does not lead to "any form of dualistic vitalism", the opposition of much modern biological thought to mechanism and materialism cannot be more forcibly stated For he himself outlines admirably the distinction between organic and all inorganic "systems" and then uses the term "vital activities" in contrast with physical ones (*ibid*, p 90) Also, the concept is explicitly stated to exclude the conscious life of Man, which must, however, be allowed for in a complete theory of biology. Yet he admits that, in Man at least, the organism can "perceive, strive feel and remember", and that "terms of psychological implication" may be used, if necessary, to describe animal behaviour, as we must refuse "to be bound or hampered by the metaphysical notion that the animal is merely a machine or can be treated as such" (*ibid*, p 91). Thus, in my opinion, the modern organism concept, as expounded by E S Russell and others, is not only valid but definitely vitalistic, particularly as regards animals, but also for plants (*ibid*, p 89). He claims to go back to the outlook of Aristotle (*ibid*, p 90).

It is, of course, essential to distinguish the study of animal behaviour by E. S. Russell and other naturalists from the "behaviourism" dealt with in III(*b*), 2, i

Dr Russell's views are given more fully in *The Behaviour of Animals* (1934) His excellent criticisms of the mechanistic methods of Pavlov and Loeb are well reinforced by an amusing quotation from Raymond Pearl (*op cit*, p. 11). There is no reason why a mechanistic formulation of behaviour problems should be accepted *a priori*; and "no such thing as pure reflex action" in the normal behaviour of an intact animal (*ibid.*, pp. 11-12) This is supported by many examples; for instance, a bird confronted with a worm swallows it if feeding itself, but carries it to the nest if it has young; and so on. "The theory that the reflex arc is the basal unit of behaviour . . . a favourite assumption of the

physiologist . . . receives little support from the facts of development Early behaviour does *not* arise through the addition and combination of originally separate reflexes." "Maturation of behaviour" too "is a phenomenon of growth and differentiation" Instinctive behaviour and learning are interwoven, as seen in an example of a young thrush breaking snail shells (*ibid*, pp 134-7)

Russell's "organismal" point of view is based on the study of animals in their natural surroundings, for this, simple naive observations are most useful, though simple experiments suitable to the abilities of the creatures concerned, such as those of Kohler on chimpanzees, are also valuable (*ibid*, pp. 9, 13-15, 154-9, and Ch. IX) With his appreciation of "good old-fashioned natural history" as an aid in getting the animal's point of view (*ibid*, pp 16-20) I heartily agree. Ecology explodes the results alleged to have been obtained from animals, often mutilated, kept in laboratories under unnatural conditions; animals are animals, not puppets, as well selected observations show (*ibid.*, pp. 24-36). For example, tropisms or "purely physiological theory" are inadequate to account for the phenomena of fish spawning (*ibid.*, pp 36-42).

Thorndyke's once influential mechanical or neural views concerning reactions in kittens and some other animals are soundly criticised. Learning is not simple reflex action but is due to conative effort; *insight* (D. K. Adams, 1931) alone shows that learning does not depend upon repetition. All learning involves a goal or expectation, and does not occur without incentive. Though theories about learning "are in a state of flux", these suggestions probably point the way to success, especially if the significance of the perceptual field, not of single items such as appeal to the mind of man, is considered when dealing with beasts (*ibid.*, pp. 150-51). This last matter is well discussed in the final chapter on "Perception (*i.e.* of whole situations) and Gestalt Theory."

"Instinctive Behaviour" is admirably considered in Chapters V and VI, with many suitable illustrations taken from R. W. G. Hingston's *Problems of Instinct and Intelligence* (London), (1928). Though "instinctive behaviour is characteristically specialised, stereotyped and little susceptible of

adaptive modification", instinct is not a special faculty, and there is no sharp line between instinctive and "intelligent or adaptive" behaviour (*ibid.*, pp 96-106) Many activities are anticipatory or, when necessary, autonomous. In fact, "behaviour is from the beginning a unified or integrated activity" (*ibid*, p. 131); separate reflexes appear later, for example in the movements of young Amblystomas and other vertebrates; and "this law holds good in the process of learning, which is a continuation of development", at least in vertebrates, "a series of organised attempts at solution", "a systematic whole" (*ibid*, pp 133-4, 147; and Lashley, p. 148).

The behaviour of animals as expounded here not only explodes mechanistic theories but demonstrates autonomy and so vitalism. The same is true of plant behaviour to a less extent (*vide* p. 237 above).

CHAPTER 2

# MODERN VITALISM: ITS VALIDITY, NATURE, AND IMPORTANCE FOR BIOLOGY

(i) *Historical introduction to arguments for the validity of vitalism.* THE successive phases through which vitalism has passed, usually characterised by the pronouncement of definite vitalistic theories, have been outlined and compared above. Often there have been corresponding periods of mechanistic belief (*cf* Part I(*c*) In Part II this was seen to be true of the nineteenth as of preceding centuries It began with vitalism, vague but mightily in the ascendant; but in the second half mechanism dominated biological thought as scientific naturalism or determinism did scientific thought in general, and this despite the vitalistic conclusion of the biogenesis controversy and the sturdy independence of such first-rate naturalists as Wallace and Fabre In Part III I have attempted to deal with the latest phase, which is dated from the experimental results published by Driesch about 1895.

His important influence upon biology has been discussed in Part III(*a*) In III(*b*) the reactions of modern philosophy, psychology and biological theory are considered; and in III(*c*) the bearing upon biology of recent developments in physical science and those national and personal backgrounds of ideas which tend to influence the theoretical conclusions drawn by biologists from their observations and experiments. In this, the final sub-Part, describing the modern revival of the old ideas of the organism and of natural history, biology resumes the primary position it should have in a history of vitalism.

A comprehensive or vitalistic science of life must be correlated with what is known in other fields of enquiry; for it touches all the "abstract" sciences on its physical side, comprises the "descriptive" ones, and must take account of the knowledge of mankind and of knowledge itself contributed

by psychology, philosophy, epistemology and similar studies (*cf* Merz, *European Thought*, ii, pp 200-216, 464, 467).

The existing arguments for the validity of vitalism may therefore be grouped as follows

*A*. 1. The Cosmogenic Argument (section ii)

*B*. Biological Arguments (section iii).

2 Biogenesis (Wallace, Gray).
3 Entropy, arrest of (Jeans, Eddington, Hopkins).
4 Harmonious-equipotential systems (Driesch).
5. Complex-equipotential systems (Driesch)
6. General autonomy and wholeness of adult organisms; the Organism or Natural History Argument (Aristotle, Wallace, Durken, J. Gray, E S Russell)
7. Animal autonomy, sensation, or consciousness (Aristotle, Wallace, McDougall, Nunn).
8 Internal organisation, enzymes, hormones, etc.; the Biochemical Argument (Hopkins and biochemists generally)
9 Various *Indicia* (Driesch mainly).

*C*. Human Arguments (section iv)

10. Human action (behaviouristic) (Driesch).
11 Autonomy of Mind (psychological) (Aristotle, Wallace, Bergson, McDougall, Jung).
12 Epistemology (philosophical).
13 Religious experience (Whetham, McDougall, Merz, Whitehead, Bp Barnes).
14 Psychic Research (Myers, Wallace, Barrett, Driesch, McDougall)

These arguments are somewhat unequal in value Some, such as numbers 3, 4, 5, 6, 10 and 11, are well known, numbers 4, 5 and 10 being used by Driesch to prove that particular vitalistic agency which he calls entelechy, though I employ them to prove vitalism in general (*cf.* III(*a*), pp. 160-1). Others, such as numbers 1, 8 and perhaps the *Indicia* in 9, have not, so far as I am aware, been used in this way before. The inclusion of Hopkins and Russell, who disclaim support of vitalism, is deliberate, as their facts prove vitalism in my sense; the tendency in some quarters to de-

preciate vitalism with mechanism is discussed in section v below. Except for Driesch's three arguments, which are due to him, the names in brackets only indicate prominent exponents of the views mentioned, whether as arguments for a vitalistic outlook or not; most of them have also been advocated by other great scientists

Number 1 alone is not an independent proof, not even a proof at all, for vitalism, considered by itself. But it paves the way for the biogenesis argument, no 2, the psychological argument, no. 11, and for the vitalistic position generally. Otherwise, the arguments are independent, and the failure of one or more could not affect the validity of the others. Numbers 13 and 14, though based on evidence outside the range of orthodox science, are placed here for convenience. The strictly scientific arguments for vitalism are therefore numbers 2-12 inclusive

It is surprising that these arguments have been previously put forth only in isolation or in small groups, thus giving a meagre view of vitalism which has evoked widespread dissatisfaction (*cf.* sec. v) The specialist has only ventured to state what seemed valid in his own sphere of embryology, psychology, or what not This combination of vitalistic arguments, to which the future may make additions, has, I feel, a unity which adds to the weight of its independent components.

(ii) *No. 1.—The Cosmogenic Argument* (*A*).

Fifty years ago T H. Huxley wrote: "It appears to me that the scientific investigator is wholly incompetent to say anything at all about the first origin of the material universe. The whole power of his organon vanishes when he has to step beyond the chain of natural causes and effects No form of nebular hypothesis that I know of is necessarily connected with any view of the origination of the nebular substance" (*Nineteenth Century*, February, 1886; quoted by Inge, *God and the Astronomers*, p. 241).

But the development of knowledge has led many of our greatest authorities beyond this simple agnosticism. Seventeen years later Lord Kelvin wrote: "There is nothing between absolute scientific belief in a creative power, and the

acceptance of the theory of a fortuitous concurrence of atoms. If you think strongly enough you will be forced by science to the belief in God which is the foundation of all religion" (*Nineteenth Century*, June, 1903; quoted by Inge, *ibid.*, pp. 233-4).

Materialistic guess-work would still like to contrive a universe that is eternal and self-sufficient. But scientific people must take their knowledge of any subject from the experts in it.

Sir James Jeans says:

"Everything points with overwhelming force to a definite event, or series of events, of creation at some time or times, *not infinitely remote* [italics mine—L. R. W.]. The universe cannot have originated by chance out of its present ingredients, and neither can it have been always the same as now" (*Eos*, London, 1928, p. 55). And ". . . entropy . . . is still increasing rapidly, and so must have had a beginning; there must have been what we may describe as a 'creation' at a time not infinitely remote." Referring to recent concepts of matter and radiation, he adds, "These concepts reduce the whole universe to a world of light, potential or existent, so that the whole story of its creation can be told with perfect accuracy and completeness in the six words: 'God said, "Let there be light."'" (*The Mysterious Universe*, London, 1930, pp. 144, 78).

Sir Arthur Eddington supports Jeans, though he would keep religion out of his science if he could:

"The beginning [of cosmic evolution] seems to present insuperable difficulties unless we agree to look on it as frankly supernatural . . I have referred elsewhere to the danger of limiting scientific investigation to a bounded domain" (*The Expanding Universe*, 1933, pp. 125 and 28; *cf.* *Science and the Unseen World*, 1929, esp. pp. 16-18).

Sir W. C. Dampier-Whetham suggests Lord Kelvin's alternative view, with a condition attached: "If infinite time is available, all unlikely things may happen" (*ibid.*, p. 483). But, he admits such suggestions "are but random speculations." For, according to all that competent astronomers admit to be true, as far as we can believe anything in astronomy is true (*cf.* Eddington, *op. cit.*, Ch. I, IV and

Ch IV, VII), the amount of time available, though admittedly very great, is limited by the universally accepted theory or law of entropy, and by the still debated theory of the expanding universe. If the universe cannot unsort or rewind itself during this large but not "infinite" interval it is doomed to reach the condition of maximum entropy and to be finished from a mechanical point of view

The "beginning" then witnessed definite creative action by God, though not necessarily out of nothing (*cf* Fleming, *The Origin of Mankind*, p 22) And as that happened for "light", including matter and radiation (*vide* Jeans above), there is no reason why it should not have occurred again for the production of life and mind, both inexplicable otherwise (*cf* arguments 2 and 11 below). A vitalistic view of life in general and mind in particular accords with that "absolute scientific belief in a creative power" which Kelvin pointed out is the only alternative to the impossible theory of "a fortuitous concurrence of atoms", or of radiation or what-not.[1]

(iii) *Biological Arguments* (*B*). *No. 2.—Biogenesis.*

This has been thoroughly discussed in II(*c*) above. As Singer says, it gives biologists "a conception of the nature of life comparable to . the conservation of matter and of energy in the hands of the physicist"; to quote Bohr again, the existence of life is an inexplicable fact that "must be taken as a starting-point in biology" Biogenesis is universally accepted by biologists under all the conditions we know about, and the unreasonableness of its hypothecation under other, surmised, conditions has been exposed by J. Gray, Tilden and others (*vide* II(*c*), iii). It is therefore an indisputable argument for vitalism Life cannot be governed by the laws of inorganic nature, as it is something *sui generis.* This argument is independent of the cosmogenic argument given above, being based solely upon biological experiments.

But, further, if life suitable for terrestrial conditions is not eternal in the universe and not an outcome of inorganic

[1] *Cf* Dunne's conclusion "that behind all physical phenomena there is the Universal Mind" (*The New Immortality*, review. *Record*, 4th Nov. 1938).

processes on the surface of the earth, I see no alternative explanation for it except a creative act of GOD.

Such admissions are repugnant to the scientific mind, which seeks, quite properly, for continuity in nature, expressed sometimes in terms of cause and effect. But the scientific mind must learn to recognise ultimate things when it comes up against them As Wallace wrote in 1889 (*Darwinism*, Ch. XV), there are clear indications of definite, so-called "supernatural" acts or periods in the history of living things, biogenesis being one of them. Such belief accords well with all we know about the "fitness of the environment"; and it agrees not only with the creation of "light" "in the beginning," but with that of the higher mental and spiritual powers of man (*cf.* sec. iv, all arguments except No 10—behaviouristic).

Alternatively, the origin of life "remains shrouded in mystery", as Henderson and some other mechanists think (*Fitness of the Environment*, pp 288, 308-10), and as Darwin said of that of matter and energy. But, if so, this mystery must not be forgotten by biologists.

*No. 3.—Entropy, the arrest of*

The "evasion" or "arrest" of the otherwise unbroken law of entropy by living organisms, has been dealt with in III(*c*), 1, ii, above. This "fundamental" attribute of life, as Sir Gowland Hopkins terms it, is a perfectly independent proof for vitalism, due largely to the exposition of astronomers and physicists, but also strongly stressed by Sir Gowland and, earlier, by Johnstone (III(*d*), 1, i).

As eventually all animals are parasitic on plants, this opposition to the second law of thermodynamics is fundamentally due to the power of plants to store up radiant energy through the peculiar properties of chlorophyll; though Hopkins points out that "autotrophic" bacteria and "purple bacteria" probably checked entropy before green plants developed (*Advancement of Science*, 1933, p. 2). The "biological" arguments for vitalism are so largely based upon animal life that this contribution from the vegetable kingdom also deserves attention as showing that "vitalism" is not to be confined to animals and Man.

But Man, alone among organisms, can "evade" entropy, to use the terminology of Jeans, by utilising and storing such secondary energies as those of wind and waterfalls, due primarily to radiant energy, and the elemental energy of radio-active bodies; and he alone employs the accumulated energy of plants for heat, while sharing with animals its use through feeding on plants or on animals which feed on plants.

*No. 4—Harmonious-equipotential systems (Driesch's first proof)*

This argument was originally worked out for animals, but it also applies to plants It is due to Driesch (*Science and Philosophy of the Organism*, A I, *B*, Ch 3, *cf* my III(*a*), ii, above), but his experiments, upon which it rests, have been repeated by T H. Morgan and other expert biologists His explanation is given in terms of "entelechy"; here I would stress that it is "a true element of nature", a proof for vitalism, for the autonomy of life (Driesch, *ibid*, p 105); and that it is quite independent of the arguments already mentioned, being derived from experimental study of embryology or morphogenesis, regeneration and restitution. The proofs are the unassailable facts which Driesch marshals under these headings in his A, Part I, *A* and *B*

Durken gives support to these facts and to their vitalistic conclusion (*Experimental Analysis of Development, e.g.* Chs. I-IV)

*No 5.—Complex-equipotential systems (Driesch's second proof).*

This argument must be distinguished carefully from Driesch's first argument, no 4 above. The former deals with "single acts" which together form a harmonious whole in development or restitution; thus, if some cells are removed from an embryo in, say, the 8-celled stage, the others will perform the functions of all the eight cells. Complex-equipotential systems function in the origins of embryos or in new formations They are well seen in plants, as in those leaves which can develop a whole plant from a bud formed from one or a few cells of the epidermis. But in animals they occur in regenerations, and notably in the origin of the generative cells from a sexual organ which itself is formed from one cell, or at most from very few. In these cases "each

single element" can perform a complex act, *i.e.* the production of a whole new individual or a large, complex part of one. As such processes cannot be described in mechanistic terms we get a distinct argument for vitalism (Driesch, *ibid*, A, I, *D*; especially pp. 144-5, 147; *cf.* my III(*a*), ii, above).

Neither of Driesch's two arguments refers in any way to that normal adult life of an organism from which independent arguments are derived in the following portion of this section.

*No. 6.—General autonomy and wholeness of adult Organisms; the Organism or Natural History Argument.*

This argument goes beyond Driesch's two proofs which deal with embryological and restitutionary development only The whole life of an organism, particularly an animal, is utterly unlike both a machine, in any sense in which that word can be fairly used, and anything found in inorganic nature This is apart from the special phenomena from which separate arguments or "*indicia*", to use Driesch's phrase, are drawn in no 9 below.

It is supported by Durken's emphasis on the importance of the whole during the development of the animal organism (*op. cit*, pp 30-3 and 250-5). J. Gray agrees with this, and adds that the "two main attributes of living matter"—inherent complexity of structure and dynamic potentiality of initiating events—are also displayed in the relationship between the adult organism and its environment (*Advancement of Science*, 1833, p. 91 etc.; *cf.* my III(*d*), 1, ii), and that the organism always behaves as a co-ordinated system "of a complexity quite unknown in the physical world", showing "properties and potentialities" not reducible to the chemical level (Russell). This leads to the exposition by E. S. Russell of animal behaviour as a "whole activity", though "simply one form of the general directive activity of the organism", and of the importance of "natural history in the old sense" (*cf.* Ch. 1 above).

All these authorities draw attention to the total inadequacy of merely physico-chemical research to eludidate more than certain limited parts of biology, and so show the inadequacy of mechanism. At the close of the previous chapter I have

indicated the reasons for taking this "holistic", "organismal" view of organisms as an independent argument for vitalism. Despite objections raised to this term by Russell because he rejects the concepts of mind and matter (*op. cit*, p 88), a rejection which I cannot accept, partly because Dr. Russell evidently has a mind, no other expression meets the case unless "life autonomy" is preferred.

Finally, we must recollect that "plants show behaviour in this general sense just as much as animals do", though only exceptionally by active movements (Russell, *ibid.*, p. 89)

*No. 7 —Animal autonomy, sensation, or consciousness.*

Closely allied with the preceding argument is the difference between plants and animals which Wallace considered the second stage in the development of the organic world (*Darwinism*, pp. 474-5) It may be granted that in the Protista there is no border-line between plants and animals; and that lower animals and parasitic forms resemble typical plants in various negative ways, such as absence of quick and voluntary movements, of power of grasping and eating bulky food, of well-developed sensory apparatus, and so on (*cf.* Johnstone, *Mechanism of Life*, p. 9).

But the fact remains that every one recognises what is meant by an animal, except perhaps a few biochemists when they are on duty (*cf* III(*b*), 3, 11—Needham) Typical Protozoa, like the Coelenterata, are clearly marked off from plants, and Jennings and others have shown that in *Stentor* and similar forms there is a power of learning and of response to unusual situations (Jennings, *Behaviour of the Lower Organisms, Science and Philosophy of the Organism*, pp 189-90, 237). And in countless representative animal species, notably among Vertebrata, Insects and other Arthropods, and Cephalopod Mollusca, there is "sensation or consciousness, constituting the fundamental distinction between the animal and vegetable kingdoms" (Wallace, *loc. cit.*).[1] This is supported by McDougall and Nunn: the former attributes "mind" to animals as well as to Man, and both consider that all animals are more or less autonomous (*vide* III(*b*), 2, iii);

[1] Vide *The Argument from Flight*, L. Richmond Wheeler (p 42, *Discovery*, Feb. 1938).

so does J B de Haan; Johnstone makes spontaneous behaviour a gradually developed property in animals (III(*d*), 1, i); and Lloyd Morgan thinks that consciousness in animals shows three grades, the lowest of which is "sentience" (Singer, *History of Science*, p 422).

Whether this developed gradually, according to "Darwinism", or *de novo* in various degrees, according to Berg (*cf.* III(*b*), 3, iii), does not affect its actual existence, best seen in the higher Vertebrates Wherever it occurs it is an argument for vitalism, additional to that derivable from the general "organismal" properties common to plants and animals dealt with in argument no 6; though it is sometimes difficult to separate the proofs for these two arguments when lower animals are under discussion

Haller's irritability of living substance may be recalled here (*cf.* p. 47 above).

*No. 8—Internal Organisation, enzymes, hormones, etc.: the Biochemical Argument*

Our increasing knowledge of the orderly production in organisms of zymogens and enzymes, when and where required, and similarly of hormones for the production of effects in parts of the organism other than their own has been considered in III(*b*), 3, iv There is nothing approaching this orderly sequence and correlation in inorganic nature, and so these form an argument for vitalism drawn from the microscopic and functional study of the body similar to those derived from the macroscopic study of behaviour and animal autonomy (nos 6 and 7).

As Johnstone has said, we cannot "explain" life as inorganic events can be and are explained by physicists; the physiologist is limited by his own methods to nothing but physical and chemical reactions, which describe but do not explain life; and "even the biochemical description (of secretion) involves at the present time hypotheses that have still to be verified" (*The Mechanism of Life*, pp. 211-12 and 240).

*No 9—Various* Indicia.

"Surplus vitality and growth-power in dominant species, and especially in the males" was first suggested by A. R.

Wallace as the proximate explanation of the superabundant glories and also of the redundancies which puzzle the observant naturalist (*cf* II(*d*), ii) Wallace elaborated this thesis concerning the gorgeous colours and strange structures of birds and butterflies (*World of Life*, pp 170-80, 261-3, 1914 edn.; *cf* *Tropical Nature* (1878), pp 364 *seq* ), and considered that it also applies to the overdevelopment of bodily bulk, monstrous horns, and so forth, which seem to have led to the extinction of "many groups of animals" (*World of Life*, pp 263-70; *cf* Smith Woodward, "Geology," British Association, 1909, and Broom, *The Coming of Man*, pp. 50-61). "Even in the vegetable kingdom this same law may have acted," Wallace considered, referring to the enormous masses of flowers and fruits often produced in excess of the needs of the species, floral coloration, the production of sap, and so on (*ibid*, pp 275-7)

I think this a perfectly sound idea, general recognition of which has been prevented among Darwinians by a too rigid adherence to the concept of utility, in which Wallace himself believed very strongly at first, with Darwin (*ibid*, pp. 319-22). The excess of poisonous substances in snakes and many plants may be due to the same principle

My own study of tropical butterflies leads to the same conclusion In addition to the excessively brilliant colouring of many, they live considerably longer than is required for the propagation of the race, and show the *joie de vivre* which Fabre noted in many less ornate insects (*cf.* II(*d*), i).

Life then in many animals and Flowering Plants exhibits excessive development in certain directions, inexplicable on grounds of utility though describable perhaps in terms of orthogenesis (*cf* III(*b*), 3, iii) This to me is an additional argument for vitalism; but as it might be explicable on deterministic lines it is placed here as an *indicium*.

Driesch, in *The Science and Philosophy of the Organism*, mentions various phenomena which he considers not absolutely irrefragible proofs for vitalism but nevertheless strong *indicia* These include:

the existence of enzymes (*op cit.*, p. 263);

instincts, which he defines as animal movements (*ibid.*, pp. 192-201);

immunity from disease (*ibid.*, pp. 136-9);
the weaknesses of Darwinism (*ibid.*, pp. 168-75);
adaptive equipotentiality (*ibid.*, p 131).

The functioning of enzymes and similar bodies has been developed here as argument no 8

Driesch's treatment of instincts is dubious and unsatisfying; he finds no argument in instincts themselves for vitalism, but suggests that *if* regulation of instincts occurred it would prove life autonomy. The existence of this "regulation", or rather of a gradual change from instinctive to autonomous behaviour, has been fully demonstrated by Johnstone, Russell, Durken and others (*q v*), and used in my argument no 7

Immunity from disease is, however, quite different from the normal functioning of hormones and enzymes in healthy organisms. Though not always successful, immunity represents a specific reaction of the organism to a specific attack, external or internal, from noxious organic substances, and so is an important *indicium* for vitalism, if not indeed an independent proof. Again, I submit, we find a phenomenon unparalleled in inorganic nature.

The weaknesses of Darwinism as a system *per se* do indeed weaken the mechanistic argument often associated with it, and their exposure is an *indicium* for vitalism. But I do not propose to stress this here, as Darwinism need not be synonymous with materialism (p 142; pp. 147-8, above), and is a useful ingredient in biological theory (III(*b*), 3, iii).

"Adaptive potentiality" is covered by Driesch's second proof and by my argument no 7.

The *indicia* which demand special consideration are therefore two—the "surplus vitality" of Wallace and the "immunity" of Driesch.

(iv) *Human Arguments (C). No. 10.—Human action (behaviouristic) (Driesch's third proof).*

Driesch's third proof of vitalism is based not on psychology, as is the following argument, no. 11, but on scientific behaviouristic study of man as a "material system in motion" and on disproof of the mechanistic fallacy that the material brain is the basis or cause for "action" as defined by

Driesch (*Science and Philosophy of the Organism*, pp. 207, 212, 217, 220; *History and Theory of Vitalism*, pp 212-13)

If it is also applicable, as I believe it is to a limited extent, to the actions of such higher animals as dogs, apes, elephants and perhaps certain birds, its valency as an argument for vitalism is strengthened. But it is desirable for historical reasons, and sufficient, to place it here Arguments from tamed mammals and birds are, to my mind, often suspect, as it is difficult to make sure that the animals are not being profoundly and abnormally influenced by the human intelligences of their owners

Anyhow, there is nothing like behaviouristic human action in the inorganic world, as Driesch says; and, at least on his historical basis, it seems a perfectly sound and independent argument for the autonomy of human life and so for vitalism, on the ground that disproof of mechanism in one species or group of organisms shows that vitalism is essential at least to that extent (*cf.* my III(*a*), ii, pp 150-2)

*No. 11.—Autonomy of Mind (psychological).*

This is another independent scientific argument, based on the study of mind as mind McDougall I consider its ablest and clearest exponent; but Bergson and Jung and other modern psychologists have with him re-established the autonomy of mind (*cf* III(*b*), 2, iii and ii) As Merz says, the vitalistic view is supported by " . the phenomena of consciousness or of an inner experience" (*European Thought*, ii, 468)

But many other scientists join with the psychologists quoted in admitting the reality and independence of mind. Johnstone did so (III(*d*), 1, i) J Arthur Thomson referred to the "epiphany" of mind in "higher animals" (E.B , 14, 42 *seq* ). Eddington says, "If the brain disturbance is translated into consciousness, we touch reality" (Whetham, *op. cit.*, p. 480). Whetham claims that "as far as direct knowledge goes, the mental world is the more real" and "there is knowledge of our own mental sensations", of which one of the strongest is that of volition and free-will (*History of Science*, pp. 475-6, 314). The validity of free-will has also been emphasised by Bishop Barnes (*op. cit.*, pp. 581-7), and McDougall (*Outline*

*of Psychology*, pp. 51, 447); and earlier by Kelvin (Merz, ii, 598).

Wallace affirmed before 1889 that one of the stages in organic evolution due to "some new power or cause," is the existence in Man of his "most characteristic and noblest faculties" (*Darwinism*, pp 473-5; *cf. Natural Selection*, Ch. X); and though this view has been reprobated by most biologists Wallace is here, as often, more in tune with modern beliefs than his materialistic critics (*cf.* II(*d*), ii, and sec. ii above).[1]

Again we note a recurrence to an idea expressed long before these writers by Aristotle (*cf.* sec v below). However far the "mental" powers of certain animals may approximate to those of Man (*cf* arguments 6 and 7), there is beyond all doubt an independence in his mind at its highest to which no parallel among the best "intelligences" of animals can be adduced.

*No 12.—Epistemology (philosophical).*

This argument is quite independent of the last one though it leads to the same result, being based upon the relatively new study of the validity of human knowledge (*cf.* III(*b*), i, iv). It is that all knowledge and theories, mechanistic or otherwise, are products of the human mind, connected, somehow, with the human body, so any valid theory about organismal, including human, bodies must allow for itself being the result of mental induction from "appearances"; so it must be vitalistic at least as far so the independence of the mind of Man is concerned.

*No. 13—Religious Experience.*

This argument comes from outside the subject-matter of science, but its relevancy is admirably stated by Whetham:

"Science must admit the psychological validity of re-

[1] *E g* F Weidenreich, reviewing the evolution of Man "in light of new discoveries", concludes that "'the developmental factors of Lamarckian and Darwinian conception are of no importance, the essence of the evolution being an independent progressive differentiation of the type itself'" (*Nature*, 10th Sept 1938, 470), and Prof H H Woollard, F R S, and J Reid Moir, F R S, in view of recent discoveries of human remains such as Mr Marston's Swanscombe "skull", incline to belief that Man is not derived from an "anthropoid" stock but "from an earlier stock" (*Nature*, 8th Oct. 1938, 673, 675, *cf.* 509–510, on 17th Sept.).

ligious experience. The mystical and direct apprehension of God is clearly to some men as real as their consciousness of personality or their perceptions of the external world." It is not necessary, nor possible, to define God. "Those who know Him will want no definition" (*op cit*, pp. 485, and xxi).

Whitehead expresses the same idea: "The *fact* [italics mine—L. R W] of the religious vision, and its history of persistent expansion, is our one ground for optimism" (*Science and the Modern World*, p 238) As Merz says, philosophic thought has now recognised that the region of artistic creation and religious thought has an independent existence and "can draw upon a fund of mental experience quite as real and inexhaustible as that . . explored by scientific thought" (*European Thought*, Vol. IV, end).

The reference to artistic creation agrees with the use made by Wallace and other wide-thinking scientists of other important human "faculties"; but it is sufficient here to consider the religious argument, admirably expressed in Tennyson's *In Memoriam* and strongly supported by *The Religion of Scientists* (1932), which gives the views of some two hundred Fellows of the Royal Society.

For those in doubt such books as Streeter's *Reality*, Barnes' *Scientific Theory and Religion*, Matthews' *Purpose of God*, and, perhaps, Fleming's *Origin of Mankind* may be useful. The alleged absence of a religious faculty in individuals no more disproves its existence elsewhere than the inability of some primitive races to count accurately above twenty or even three disproves the existence of the mathematical powers of such persons as Euclid, Newton and Pascal, or the validity of mathematics (*cf.* III(*c*), 2, ii and iii, concerning subjective idiosyncrasies of biologists).

Here is no question of the evolution of religious ideas, which, like the evolution of magic and of scientific ideas, can legitimately be treated scientifically. The evidence is that, apart from man's varying conceptions, ceremonials and creeds, there eternally is a Supreme Transcendent Being (*cf.* sec. ii above). Man's ability to obtain communion in any way with Him is a matter different in quality from the highest intellectual or conative achievements. No organism

capable of this is properly accounted for by a mechanistic philosophy or science (*cf* McDougall's *Religion and the Sciences of Life*, 1934) So this is an independent argument for the validity of a non-mechanistic view of Man, and so for vitalism (*cf.* close of argument no. 10).

*No. 14.—Psychic Research*

For many years psychical research has been conducted on scientific lines by scientists and other intelligent and reputable people; and I agree with C D. Broad that its evidence for many abnormal events is not to be lightly dismissed by scientific investigators of the nature of Man (*The Mind and its Place in Nature*) This evidence has often been disregarded on *a priori* grounds by those who have not studied it (Wallace, *My Life*, Chs. XXXV-XXXVII) Broad has written admirably in this connection about much "science" being often only the very fallible opinions of Professor X and his circle of friends (*op cit*, p 515).

Here I would only claim that the evidence for such phenomena as telepathy and, perhaps, "Phantasms of the Living" is sufficiently strong to establish mental or "psychic" powers for Man, at least for some men and women, which cannot be accounted for by any mechanistic theory (*vide e.g. Psychical Research*, Driesch, 1933), and therefore form another proof for a non-mechanistic view of Man and so for the vitalistic position generally.

Among the first-rate scientists who have accepted some such position as this—though many of them go much further—may be mentioned C Flammarion, Sir W. Crookes, A. R. Wallace, Sir Oliver Lodge, Sir W. F. Barrett, and Professor Driesch; to whom the names of F W. H. Myers and his colleagues should be added. One of the latest scientific authorities to appreciate the importance of psychic research concerning "the position of minds in the universe" is Professor McDougall ((*Religion and the Sciences of Life*, 1934, Chs. IV and V)

One objection to the validity of these phenomena or to evidential use thereof is that, at the best, they are comparatively rare and cannot be produced at will. But the latter remark applies to many important observations in

biology (*cf.* Driesch, *Science and Philosophy of the Organism*, pp. 2, 3); and the former criticism also applies to the occurrence of genius and other exceptional human qualities Yet these are admitted to be suitable matter for scientific study by Galton and other writers on eugenics.

(v) *The nature of modern vitalism, and its importance for biology.*

It is clear that the specific vitalistic ideas expounded in the past, for example by Stahl or Berzelius, are no longer tenable. They were often incorrect in themselves; but, further, no single exclusive theory is adequate for the enormous variety of phenomena with which biology now deals. This last criticism also applies to such theories as the entelechy of Driesch, invaluable as his work is (*cf.* III(*a*), iii) So it is common to find "vitalism" rejected even by biologists who realise the inadequacy of mechanism

Thus, Nordenskiold regards mechanism and vitalism as failures and thinks "resignation" would be wiser than either (*History of Biology*, p. 612). Woodger points out that much criticism of vitalism is beside the point; yet he too thinks there is little to be said for vitalistic theories, though more critical of mechanism (*Biological Principles*, pp. 266-7, 325). J A Bierens de Haan also rejects these two alternatives in a suggestive article (*Science Progress*, October, 1935) Merz is, however, strongly for vitalism against mechanism (*ibid*, iii, 393, etc ); so is Singer (*e.g* *History of Biology*, Preface, p 429, etc.). Yet Whetham, whose *History of Science* has been frequently quoted in these pages, says of vitalism, "This idea is discredited nowadays" (*op cit*, p xx); though it is specifically supported by Driesch and McDougall and implicitly by the vast band of scientists who reject mechanism. In fact, all these criticisms seem to me to be directed against special vitalistic ideas and to fail against such a combination of the arguments for vitalism as is given above. These seem to me to include de Haan's three "tendencies" and to give a more active view of the "resignation" outlook of Nordenskiold; and, as regards Woodger's criticisms, besides covering biological data they include suitable reference to the data of mental experience.

The general inadequacy of mechanism as a description of

life phenomena has been summarised in the arguments in sections iii and iv above, and demonstrated in respect to particular expositions in Chapters II(*b*), iv and v; III(*b*), 1, 2 and 3; III(*c*), 1; and III(*d*), 1. In particular, the attempt of J Needham to define biology as a mere subdivision of chemistry and physics has been criticised in III(*b*), 3, ii. It is like an edition of *Hamlet* in which all references to the Prince of Denmark are expunged; after which we are informed that there is no Hamlet in it, despite the peculiar name.

There remains one plausible modern view, that the mechanist-vitalist controversy is a thing of the past, a matter of narrow views based on imperfect knowledge, which will be replaced by a broader synthesis founded upon new and wider definitions This has been expounded by C. H. Waddington (*Discovery*, May, 1935). The way out of the controversy, he thinks, is provided by "the idea of organisation or arrangement in a pattern"; and he quotes as an analogy the increased knowledge gained in physics from study of the "patterns" found in electron-photon combinations in atoms.

Sir Gowland Hopkins used somewhat similar language in his British Association address (*Advancement of Science*, 1933), though in other respects it was largely vitalistic (*cf.* III(*d*), 1, ii). But Mr. Waddington is much more precise; all that biologists have to do, he thinks, to avoid introducing vitalistic ideas is to alter the definitions of physico-chemical concepts. And, "when we have discovered what particular arrangement of the fundamental physical elements gives rise to consciousness, we shall be able to add the necessary property to our present definition of the physical concept." This, of course, is rank mechanism, and clashes hopelessly with the independence of the mind which is established by McDougall and others (*cf.* III(*b*), 2, iii, and arguments 10-14 above). Yet the remainder of Waddington's paper, admitting that a new order of arrangement of living material arises during animal development and that, though function is often made possible by structure, we do not know how the structure is held together, is either congruent with vitalistic conceptions or seems to demand them!

This also applies to the brilliant expositions of animal behaviour of E S Russell, whose only objection to the use of "vitalism" is that he rejects the concept of mind (*cf.* argument 6 and III(*d*), 1, ii) These advocates for the abandonment of vitalism adduce proofs like those of Driesch, Durken and Wallace to show that mechanism is inadequate, and object to vitalism for reasons which psychology and their own mental powers show to be quite fallacious (*cf.* my "Fallacy of Misplaced Simplicity", III(*b*), 1, ii).

No vitalist now challenges the fullest possible application of physical and chemical methods to biological problems, nor the reasonableness of checking biological theories by the proven results of these methods But there are various planes of organisation in nature (*cf* III(*c*), 1, i), and it is hopeless to try to explain one plane wholly in terms of another.

As Sir J Arthur Thomson, who saw no need for a "mysterious vital force" nor for absolute belief in biogenesis, said, life is a "unique kind of activity", requiring "concepts transcending those of mechanism", and "mind" is independent (E B., art *Life*, 14; *cf* Biology, 3) To these fundamentals I would add the inexplicable origin of life, its unique resistance to entropy, powers of reproduction, heredity, development and restitution, and other phenomena enumerated above.

Modern vitalism stands, or should stand, for everything in biology that is not explicable by inorganic or abstract science, for everything proved by arguments in this chapter. Firstly, all living things possess a common principle, which I hold with Bohr should be simply termed "life", as occurring in them uniquely and impossible of definition in more elementary terms This, of course, is the vegetative "soul" of Aristotle. But in addition animals appear to possess, though gradually no doubt, a further power of autonomy which may be called, as by Wallace, sensation or consciousness, which is Aristotle's animal "soul" or "sensation" (*De anima*, II, 2, para. 4, etc.). Finally, the evidence that mind, in Man at least, is independent or autonomous seems conclusive (*cf.* III(*b*), 2, iii, and arguments 10-14). So we not only return to the three grades of the vitalism of Aristotle

(*cf.* I(*a*), 1, i) and of Wallace (II(*d*), ii), which is very remarkable, but confirm the common belief in life as a peculiar autonomous characteristic of living things. Thus Driesch's *dictum* that the "assertions" of Aristotle have been justified by recent research (*cf* I(*a*), 1, i, end) is largely correct, though, of course, they have been modified and some even refuted by modern knowledge This verification of Aristotle's vitalistic ideas is the more striking because I previously considered the modern chorus of praise for his biology excessive and inclined to traditionalism, and after writing my opening chapter I simply followed the argument along all the lines opened up by modern investigators And then the conclusion comes out largely on Aristotelian lines!

That life and mind are elementary facts not derivable from inorganic processes is confirmed by the cosmogenic argument that matter or at least energy must have had a special genesis (III(*d*), 2, ii); so the universe is not automatic, and the occurrence of one or more big geneses or breaks in its continuity makes others not unlikely.

It is sometimes stated, as by Sir M. Foster and Johnstone (*Mechanism of Life*, p 160), E. B Wilson (Woodger, *op. cit.*, p. 232), and Sachs (*History of Botany*, III, Ch II), that progress in biology has occurred mainly during mechanistic periods, though Sachs is careful to say that this was due to vital force being regarded as a substitute for, not an accessory to, physical forces But the period he refers to, *c.* 1804-1822, is not a good example, because great advances in plant physiology had been made just before when vitalism was equally dominant (*cf.* II(*a*), i).

On a wide view I consider that advance in details has often been promoted by a mechanistic outlook This is seen in the impulse given to biology by Darwin's theory and in the zeal with which physiologists tackle their problems. But similar work may be done equally well and keenly by vitalists like Wallace, J. Muller, and J. S. Haldane; further, devotion to details which seem to support mechanist views often leads to neglect of other fields of research and to blindness of interpretation (*cf* III(*a*), ii and iii; III(*b*), 3, v), and to useless expenditure of energy on attempts to manufacture "living cells", and so on (*cf.* II(*c*), iii). But the principal argument is

that under pure mechanism biology is pitifully biased and restricted in scope (III(*b*), 3, ii); and consequently its reaction upon sociology and morals is misleading and potentially productive of evil (*cf* Huxley, Romanes Lecture; Woodger, pp 472-5). A similar conclusion is well expressed by Woodger (*Biological Principles*, pp 232-3, 323).

But a vitalistic outlook is likely to promote advance in every department of biology, not in one or two only. As I see it, modern vitalism stands for broad views in biology, accepting all that other branches of science can offer but employing, where necessary, distinctive, fundamental, concepts, not derivable from the material of inorganic science; and most certainly rejecting the claims of mechanism to "explain" all the phenomena of life in general and of animals and Man in particular. Here I would turn again to Whetham, despite his low opinion of "vitalism" as he sees it, for a clear expression of the meaning of the history and philosophy of science—"observation or experiment is the starting-point of the investigation and the final arbiter" (*ibid.*, p xiv).

For biology to be a great branch of human knowledge, "the mistress and not the servant of physics or of chemistry", comprising a sane, balanced study of all living things and all their activities and properties, the vitalistic mode of outlook is vital. And biology is a great branch of human knowledge.

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